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THE GRAPHOPHONE AND PHONOGRAPH.

TOWARD the close of 1877, the public mind, which for twelve months previously had been fairly overwhelmed with the discoveries of scientific investigators, was again excited by the announcement that Mr. T. A. Edison had discovered a method of mechanically reproducing speech by means of an instrument which could record, and afterward repeat, messages spoken into it. Under the name of the phonograph this most ingenious apparatus speedily became familiar to every one, and during the Paris Exhibition of 1878, it, with the telephone, ranked among the principal attractions. With the profound and simple faith that was so prevalent at that time in all which emanated from Mr. Edison's laboratory, a great and useful future was predicted for the invention. Unfortunately, events did not justify this sanguine forecast; the phonograph quickly descended to the level of a scientific toy, the monopoly of which had been acquired by an enterprising London firm. Later on, in the course of a famous series of lawsuits, it was found that Mr. Edison's patented claims for the phonograph in this country were included in another and more important patent, the validity of which was thereby jeopardised, and Mr. Edison did not hesitate to cut off and cast out the offending member, in order to save the rest of his patent alive. Since that time until recently, we have heard but little of the phonograph; but some few months ago it was confidently stated that the great inventor, having now more leisure on his hands, had again turned his attention to his old speaking machine, and that very soon the world would be startled with a new phonograph which would find a use almost as universal as the telephone. In due time the new phonograph arrived in this country, and the wonderful ingenuity which had been displayed in perfecting it, as well as the clearness and precision with which it reproduced spoken records, appeared to justify the claims, in part if not in all, that had been made for it. Instead of the phonogram being indented upon a sheet of tinfoil that had been cemented upon a spirally grooved drum traversing on a screw, the record was engraved upon a small and compact cylinder of paper covered with a film of wax. Under the able management of Colonel Gouraud, Edison's new phonograph has been a source of great interest to a large number of persons, who, almost without exception, express themselves as delighted with its performance and sanguine as to its usefulness.

A formidable rival has, however, lately made its appearance. This is the graphophone, an invention of Professor Graham Bell, Dr. Chadwick Bell, and Mr. Charles Sumner Tainter, of Washington. The instrument is the outcome of a long series of experiments carried on for several years, and in its present form, if it does not justify all the practical anticipations of its inventors, they can at least claim for it the merit of extreme simplicity and ingenuity, combined with a really wonderful efficiency in recording speech and musical sounds.

Commencing with a description of the phonograph which is illustrated by Fig. 1, it will be seen that it is mounted on a hollow wooden base which contains an electro-motor, the details of which have not been made public in this country. The spindle of the motor rises through the top of the base and carries two pulleys, on each of which there runs a band. The upper pulley drives a governor which can be adjusted to give any required number of revolutions per minute within limits. The lower pulley drives the phonograph itself, the belt running round a pair of guide pulleys on to the main driving pulley and also round a subsidiary driving pulley on a screw, which is brought into action when it is required to repeat some of the words which have just been uttered by the instrument. The spindle on which the main driving pulley is fixed is carried in two bearings; the part between the bearings is very finely screw-threaded (these threads are not shown on the engraving), while the part which overhangs carries a taper brass mandrel on which the wax cylinder which receives the record is slipped. The fine-threaded screw serves to give the feed to the diaphragms, carrying them lengthwise of the wax cylinder, so that the style traces a helix which is of the same pitch as the screw. In engagement with this screw is a part nut attached to a lever which projects from a frame guided on a slide at the back of the machine. This frame also carries the diaphragm which moves with it.

There are two diaphragms—the first of glass for receiving and recording the message, and the second of silk for interpreting the record and articulating it afresh. They are both fixed in a plate jointed to the back frame. By moving the plate through about 90°, either diaphragm can be brought into the acting position, the other standing aside idly the meantime; in the engraving it is the articulating diaphragm which is shown over the cylinder. Both diaphragms are inclosed in metal cases having openings to which flexible tubes are connected. The listening tube is bifurcated, and at each extremity carries a small bent nozzle which rests easily in the ear. The other tube, which is shown lying beside the instrument, is very similar to those used in speaking to deaf persons. In the center of each diaphragm is a style, the one for engraving being stiff and sharp, while the other is hook-shaped, so that it drags over the record without any tendency to cut down the elevated portions. The frame which carries the diaphragm can tilt on the back guide, its weight being carried by a set screw sliding along the rail in front of the cylinder. By means of this screw the styles can be adjusted exactly in relation to the wax cylinder. The rail is not fixed rigidly, however, but is carried by a cam by which it can be raised at will, the cam being turned by hand, or in some cases by the foot of the operator. A partial movement of the cam lifts the style clear of the wax cylinder, and at the same time tilts the back frame, lifting the part nut off the screw. Thus the instrument is thrown out of action. By turning the cam still further, a finger on the nut lever is brought into engagement with the comparatively coarse-threaded screw shown in front, and then the frame with the diaphragms is moved rapidly back. The object of this arrangement is to enable a clerk who is writing out the record, as if from dictation, to repeat a sentence if he should desire to do so.

The wax cylinders are 2 in. in diameter by 4¼ in. long. They are slightly taper inside to fit upon the mandrel. Their thickness depends upon the use to which they are to be put. If they are intended to be sent through the post, they are formed of a paper tube with a thin skin of wax over it. If they are intended to take the place of a shorthand writer in an office, the wax is of considerable thickness (see figure), and after it has once served its purpose its surface is skimmed off. To enable this to be done with ease there is attached to the under side of the plate which carries the diaphragm a cutting tool which always precedes the engraving style and trims up the wax surface in front of it. One cylinder will serve for more than forty successive records. The pitch of the guide screw is one hundredth of an inch, and its speed of rotation 60 revolutions per minute. The style, therefore, passes over some 380 in. a minute, which gives about 3 in. for the record of each word uttered by a deliberate speaker. For recording music the speed has to be increased to 100 revolutions, and that speed has also to be used when it is desired to make the articulation audible without the use of ear tubes. The fineness of the helix renders it almost certain that the articulating style will fall into the groove as soon as a cylinder is placed in the mandrel, but if it does not do so, a slight turn of an adjusting screw puts the diaphragm into the correct position.

The illustration (Fig. 2) gives a very clear idea of the general arrangement and the construction of the Bell-Tainter graphophone. As will be seen, the instrument is mounted on a table provided with a lid which can be closed and locked when not in use. Underneath this table is fixed a balanced treadle and driving wheel, similar to those of a sewing machine. The cord from the driving wheel passes through the table and round a small pulley, *b* (Fig. 8), to actuate the governing device, which is a very ingenious piece of mechanism, and will be described in detail later on. Meantime it may be remarked that it is arranged to give a speed of 160 revolutions per minute, and that in practice this speed can be maintained within one or two revolutions, no matter how fast or how irregularly the treadle is worked. From a pulley, *c* (Fig. 8), on the other end of the governor, a cord passes to the main pulley of the instrument, which is fixed in front of the table. This pulley, *b* (Fig. 2), is loose on a spindle which carries the centering drum that supports one end of the record cylinder. A similar drum, *c* (Fig. 2), opposite the former, and at a distance from it corresponding to the length of the cylinder, runs free in a suitable bearing. This drum and its spindle are capable of a lateral motion controlled by a spring. To mount a cylinder upon its centers, the drum, *c*, and its spindle are drawn back, the cylinder is put in position, and the spring is released, so that the cylinder is held tightly by its bearings, and any motion communicated to the driving pulley through the governor, *a* (Fig. 2), is of course imparted to it. In order to provide a means for starting or arresting the movement of the cylinder instantly—an operation absolutely necessary in operating the graphophone—the driving pulley of the instrument, which, as has already been mentioned, runs loose on its spindle, can be made fast with the latter at will by means of a clutch, *b'* (Fig. 2), which can be thrown in and out of gear by means of a system of levers operated by two buttons, *d* (Fig. 2), placed in the position shown in the

FIG. 2.—THE BELL-TAINTER GRAPHOPHONE.

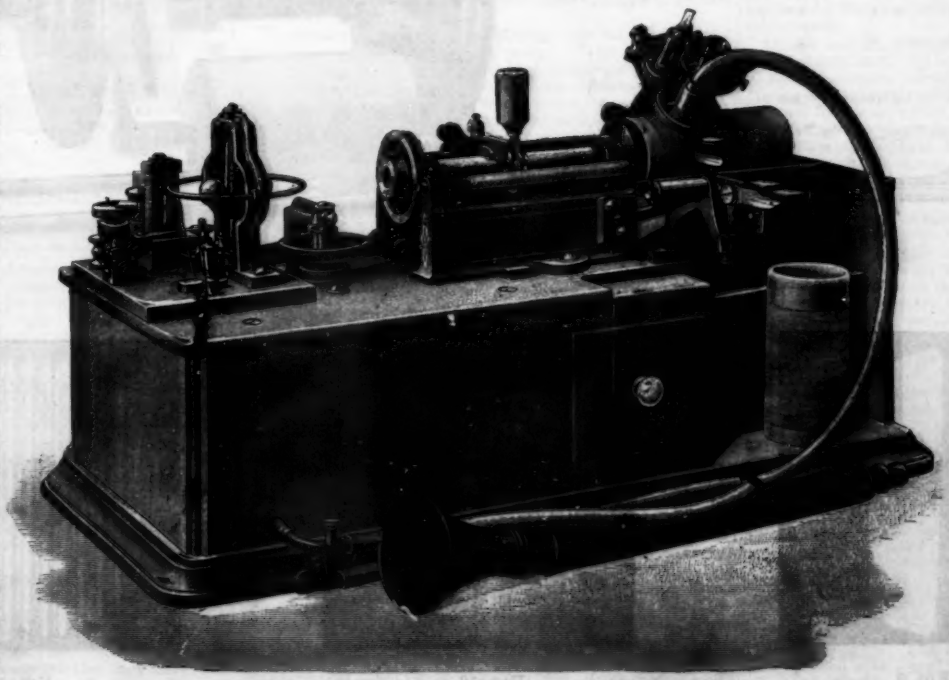

FIG. 1.—THE EDISON PHONOGRAPH.

engraving; by depressing one or other of these buttons or keys, the record cylinder can be stopped and started instantly.

The engraving style is made to traverse in contact with the cylinder from right to left. It is carried upon a tube which is fixed parallel to the cylinder, but at a higher level, as shown in the general view. The lower part of this tube for nearly the whole of its length is cut away, so as to expose a very fine-threaded screw placed within it and running in bearings at each end of the tube. This screw is caused to revolve by means of toothed gearing driven from the main pulley, so that when this latter is running idle, the screw, as well as the record cylinder, is stopped. The device carrying the style is shown in the general view, as well as by the detailed sketches, Figs. 3 and 4. The circular box, *a*, containing the recording diaphragm, is carried at the end of a short arm which terminates in a half sleeve, *f*, of the same diameter as the tube inclosing the screw. Hinged to this half sleeve is another similar one, f^1, from which, in the middle of its length, projects a short arm carrying at its end a relatively heavy balance weight, *h*. Set in a slot made in the half sleeve first referred to is a portion of a nut, *g*, threaded to the same pitch as the screw; at the back of this nut is a spring which keeps it projecting slightly beyond the face of the sleeve, but which allows it to pass back into the recess if a slight pressure be applied. From this description and from the engraving, it will be readily understood that if the sleeve is opened and placed upon the tube, the weight of the recording box and of the counterbalance keeps the sleeve fast in its proper position, while the light spring before mentioned holds the threaded block up against the screw, and when the latter is caused to revolve, the sleeve and the recording apparatus it carries traverse from left to right. The position of the various parts is so arranged that the style attached to the center of the diaphragm slightly penetrates the wax film with which the record cylinder is coated, and in this way a very fine screw of 160 threads to the inch and one thousandth of an inch in depth is traced upon the cylinder. We now come to consider the construction of the recording part of the instrument, the function of which is to receive the sound vibrations and to engrave them faithfully upon the wax surface. A section of this part of the instrument is shown in Fig. 4. It will be seen that it consists of the shallow circular box, *a*, referred to previously as being attached to the carrying sleeve. The diaphragm forming a bottom to this box is made from a piece of very thin and flawless mica; a short distance above, and parallel to it, is fixed in the box a metal plate, *b*, pierced with two series of concentric slots, b^1; above this again, but not in contact with it, is a metal cone, *f*, the center of which coincides with the center of the box, and is therefore immediately over the style attached to the mica. All these parts are inclosed within the box by a metal cover with a central opening, *g*, to which is attached a flexible speaking tube provided with a mouthpiece. In front of the mica diaphragm, and stretching from one side of the box to the other, is a metal bridge, *e*, so placed that it is almost in contact with the style. In the center of this bridge a projection is formed of such a shape that when the instrument is in operation, it presses upon the wax surface of the recording cylinder and burnishes it in advance of the style, so that the latter may have an absolutely true surface to work upon. The form and relative positions of the various parts of this portion of the instrument are clearly shown upon the engravings.

Fig. 7.

The style, *c* (Fig. 4), upon which the efficient working of the graphophone depends, although of very minute proportions, has to be shaped with the utmost precision. It is simply a very fine chisel-pointed cutting tool capable of forming a perfect thread upon the wax-coated cylinder, of the pitch and depth already mentioned. In engraving a communication with this instrument, the carrier is placed upon its tube so that the style is bearing upon the cylinder; the driving pulley is set in motion by pressing the starting key; the message is delivered clearly and distinctly through the mouthpiece of the speaking tube, and the air vibrations thus created strike upon the cone within the receiving box, and are distributed uniformly over the surface of the mica diaphragm with the aid of the slotted plate, b^1, setting up in the latter a series of vibrations corresponding to the sounds produced by the speaker and transmitted through the tube. Fig. 7 is an exaggerated section of the style and cylinder, showing the former cutting the spiral grooves; the vibrations of the style, transmitted by the pulsating diaphragm, producing the waves at the bottom of the grooves, which correspond with the spoken communication. It will be seen from Figs. 3 and 4 that the bridge piece which crosses in front of the diaphragm is attached to the bottom of the recording box by means of two projections on the latter; one of these projections is a fixture with the box; the other (*d*, Fig. 4) passes through an opening made in it, and is held in place by an adjusting screw. By slackening or tightening this screw, a slight range of motion can be given to the bridge, and in this way the relative positions of the burnisher and the cutting style can be modified. The illustration, Fig. 2, shows a double mouthpiece attached to the receiving box. In general use only a single speaking tube would be employed, but a double one is also provided for the purpose of recording conversation, etc.

We have now to describe the transmitting or repeating mechanism, which is illustrated by Figs. 5 and 6. It consists of a light carriage, *a*, for carrying the socket, *f*, to which the transmission tube is attached, as well as the diaphragm and its attachments. On this carriage are four curved arms; the back pair, *b*, fixed, and the forward pair, *c*, hinged to the carriage and controlled by springs. A threaded block or nut, *d*, similar to that already described as forming part of the receiving mechanism, is fixed between the forward pair of arms; at the back of the carriage and rigid with it is another pair of arms with connecting pieces at top and bottom; this serves as a handle for holding the transmitter when it is taken off or put on the instrument. The front part of the carriage terminates in a screwed tubular socket which forms a continuation of the nozzle on which the elastic transmitting tube is fixed; upon this socket is screwed the circular box, *e*, containing the transmitting diaphragm. The under side of the box is pierced with holes to prevent the setting up of air currents within, which might interfere with the proper action of the diaphragm. A hollow stem, *g*, terminating in a curved beak, g^1, of the form shown, forms a part of the bottom of the box. To the center of the diaphragm, e^1, Fig. 6, which is of mica, is attached one end of a silk thread, *h*, the other end being fastened to a small curved style, *i*, which is secured to the beak by a pin in such a way as to give it entire freedom of motion. The section, Fig. 6, will explain this arrangement and the relative positions of all the different parts. If now a record has been engraved by the recording style upon the wax-coated cylinder, and the recording diaphragm with its attachments has been removed, the transmitting carriage is slipped over the tube containing the traversing screw. In doing this the nut between the front pair of arms engages with the screw, the point of the curved style enters the groove engraved upon the cylinder, and on the instrument being

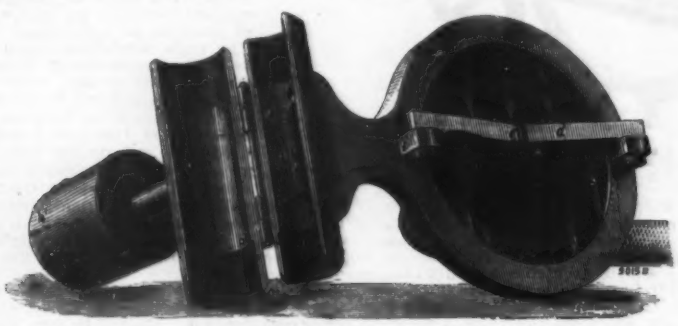

Fig. 3.

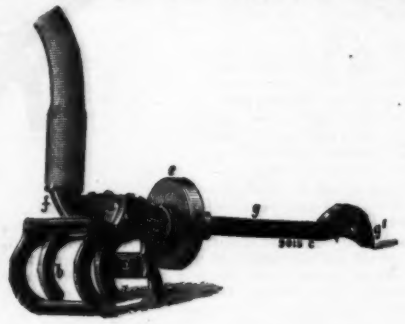

Fig. 5.

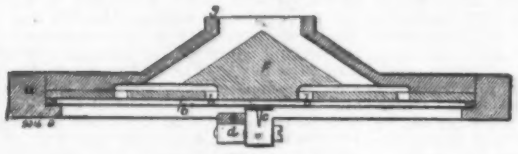

Fig. 4.

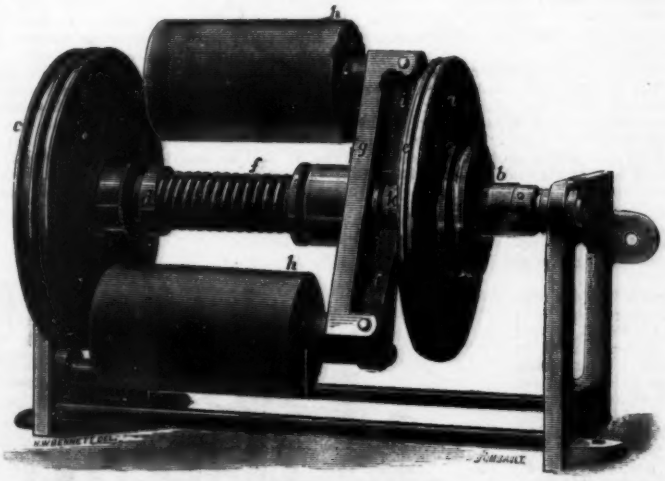

Fig. 8.

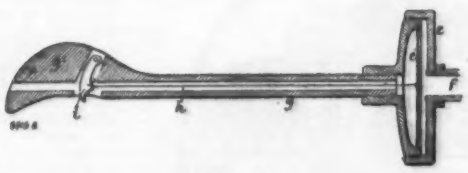

Fig. 6.

Fig. 9.

Fig. 10.

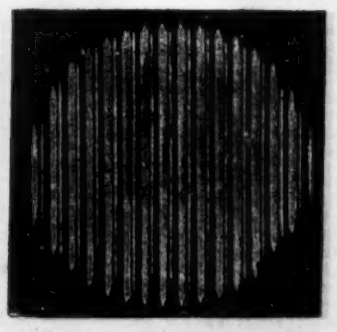

Fig. 11.

THE GRAPHOPHONE AND PHONOGRAPH.

set in motion the irregularities which had previously been engraved by the recording style give a corresponding motion to the transmitting point, and by means of the silk thread which is kept in tension set up in the transmitting diaphragm a series of vibrations similar in character to those which had been previously created in the recording diaphragm by the message spoken into it. In this way the original sounds are faithfully reproduced as to quality, but not as to intensity, perhaps owing to the smaller diameter of the repeating diaphragm, but they are not audible excepting through the intervention of the transmitting tube, which is shown lying on the table of the instrument in Fig. 2. This tube, which is slipped over the nozzle, *f*, Fig. 6, is bifurcated near its outer end, the branches terminating in curved ear pieces that are hung upon the ears when the instrument is being used for repeating.

The governor, which maintains a constant speed of the record cylinder and the feed screw, is shown in Fig. 8. It consists of a light frame secured to the table, and carrying a spindle on which the device is mounted. Loose on the spindle near the right hand end of the frame are a disk, *a*, and pulley, *b*, made in one piece; a belt from the treadle passing over *b* drives the governor. The inner face of the disk is covered with leather. The driving pulley, *c*, which gives motion through a belt to the instrument, is fast on the spindle, and is formed with a boss, *d*, on the inner side. A third disk, *e*, corresponding to *a*, is held in contact with the leather facing on this latter by a strong spiral spring, *f*, abutting against the boss of the pulley, *c*, and a disk close to the cross-arm, *g*, keyed to the spindle. Pinned to the ends of this arm are the two weights, *h h*, and two short arms, *i i*, project from them at the point where they are pinned to the cross arms, *g*, the end engaging in a groove formed in the boss, *k*, of the disk, *e*. Two small pins pass from these arms through the boss, *k*, on the arm, *g*, and into the disk against which the spiral spring, *f*, presses. It will be seen that this spring holds the disk, *e*, in close contact with the disk, *a*—sufficiently so that, when motion is transmitted from the treadle to the pulley, *b*, the governor is caused to revolve, and a belt from the pulley, *c*, to the instrument gives the desired motion to the cylinder and driving screw. So long as the speed continues normal, the instrument is driven at the rate for which the different parts are arranged, but should an extra velocity be given, the weights of the governor open slightly, and the pressure between the disks, *a* and *e*, is reduced so that the speed falls instantly. So nicely are the various parts adjusted that, with the most ordinary care, the normal rate of 160 revolutions per minute, to which the instrument is speeded, need never be exceeded by more than one or two revolutions.

The paper cylinders upon which the records are engraved are manufactured with great accuracy. They are made of two thicknesses of paper cut into strips and rolled spirally upon a mandrel. The lower strip, which is of a thicker material than the upper one, is so coiled that the edges butt and do not overlap; the edges of the upper strips also butt, the joints lying midway between those of the lower thickness; paste is used to cement the two thicknesses together. After the cylinder is thus formed the outer surface is covered with a coating of wax so blended with other materials that it does not soften at any moderate temperature. The surface thus prepared is very smooth and uniform, but it requires to be submitted to the burnishing operation already described, to fit it to receive the engraved record. A few accessories to the instrument, and which are shown in Fig. 2, remain to be described. These are: First, a tray placed beneath the cylinder to receive the fine cuttings removed by the style from the surface of the wax. Secondly, a stand to which are fixed a number of small drums just large enough to fit one end of the cylinder. This is a convenient method of storing a number of cylinders on the table of the machine. Thirdly, the device for sending the engraved record through the post. For this purpose a box just large enough to hold a cylinder is employed, the box being provided with a sliding and reversible cover so that it can be sent backward and forward through the post between two correspondents, any number of times. To protect the wax cylinder from being rubbed, loose ferrules with enlarged ends are slipped into each end of the cylinder, so that the latter does not come into contact with the box.

Figs. 9, 10, 11 are reproductions of micro-photographs from a record made on the original Edison phonograph of a record on the modern phonograph, and from the graphophone, respectively.

The phonograph and the graphophone have formed the subjects of two papers read before the British Association at Bath, by Colonel Gouraud and Mr. Henry Edmunds, the respective representatives in this country of Mr. Edison and of the inventors of the graphophone.

Both were shown in the rooms adjoining the hall. Colonel Gouraud had one instrument and Mr. Edmunds two. One of the latter appeared to us decidedly superior to the other, but, taking the best of the two, we could not see that there was anything to choose between the phonograph and graphophone. Both are truly marvelous creations, at least they appear so to every one now with the novelty still fresh upon them. They will play, sing, whistle, talk, sneeze, cough, in fact, there seems no sound beyond their powers of reproduction, and what can be said of one we think may be said of the other.

The great difference of course is in the method of working. The graphophone is fitted with a very sensitive and beautifully designed speed governor, and it can therefore be worked by human power. The agency selected is that of the foot, a treadle like that used with a sewing machine being used. In fact, the whole thing might easily be mistaken, if looked at casually, for a new form of sewing machine.

But although these instruments will reproduce nearly every sound, they do not give the tones in all their purity. There is what may be called a phonos intonation. With the old phonograph, when tinfoil was used, this was very noticeable, so much so at times as to render the instrument almost unintelligible. The new machines are a wonderful improvement in this respect. Putting aside the applying ear tubes to the ears, no one, we think, could fail to recognize the fact that the sounds were being transmitted by one or the other of these instruments.

It is sometimes supposed that the phonograph can reproduce instrumental and vocal music in miniature, but with all the charm of the original. Mr. Preece says that a symphony of Beethoven's has been so exquisitely rendered by the machine as to make him shed tears; but then Mr. Preece is, as every one knows, of a very generous and emotional nature. Our own experience does not quite bear out these high expectations; still, it is easy to detect, for instance, the difference between the performance of a piano organ and a hand-played piano. In a duet between a violin and piano one can hear when the piano accompaniment stops and the fiddle alone carries on the music. The fiddle, it may be remarked, is one of the most difficult, if not the most difficult instrument, to "take off," to use a reporting phrase, and the sounds representing its music are certainly not pleasing.

With regard to ordinary speech, we speak only from our own experience. At first we were certainly not able to follow the words on the phonograph, but, as it afterward appeared, the conditions were exceptionally unfavorable, and subsequent experience proved that the perfected phonograph, like the graphophone, is capable of repeating long sentences with distinctness and accuracy. It will be proved in time, when the instruments come into general use, how far they will be reliable under varying conditions—that is, what percentage of spoiled and inarticulate cylinders there will be.

A curious fact has been brought out by the invention of these instruments, and that is how few people (if any) know the sound of their own voice. For instance, if a number of people speak into the machine one after the other, each one may be able to recognize the voice of his friend or neighbor, but fails to identify his own. A like phenomenon in the matter of sight is often observed by photographers, who frequently can give satisfaction in a likeness to every one but the original, all which goes to show that not only do we not "see ourselves as others see us," but we do not hear ourselves as others hear us.

As in listening to conversation through the telephone, so there is a good deal of skill or "knack" required in hearing these new instruments. A great deal of this is mental. One cannot at first put one's self in the attitude of expecting speech from a machine, and it is only gradually that it dawns on one's consciousness that articulate sound waves are exercising their influence on the ear; there seems to be a sort of hang fire between the brain and ear. This, however, has nothing to do with the instrument, but is dependent on physical conditions, excepting in so far that the phonographic speech, however loud, appears to come from a distance, and until the fact is recognized, one attunes one's ear for utterances close by.—*Engineering*.

REDUCING THE DENSITY OF NEGATIVES BY VARIOUS AGENTS.*

By Dr. CHARLES EHRMANN.

OVER-DEVELOPED negatives yield hard prints, no matter how long sensitized paper may be exposed to light under them; but such negatives can be reduced to normal density, either locally or over the entire surface. To do this properly is as easy and as difficult as any of the important photographic operations requiring skill, circumspection, and a close observance of the process while it is going on.

When we first became acquainted with gelatine emulsion plates, owing to want of practice and experience many errors were made in the development, and in the majority of cases we produced but very feeble negatives from which, to make good printers, we learned to intensify. Others, however, were so excessively intense that printing from them in a reasonable space of time was quite impossible. To reduce undue density we resorted, in the beginning, to an old remedy that had served us well in the collodion process—a strong solution of cyanide of potassium, to which was added, occasionally, a trace of iodine.

To a certain degree this remedy proved to be quite efficient, but as its action is slow, and commercial cyanide of potassium invariably contains as much as from twenty to twenty-five per cent. of carbonate or hydrated oxide of potassium, the gelatine film weakened under its action to such an extent that it sometimes even became detached from the glass plate entirely, resulting, of course, in a loss of the negative. The method might answer, however, if a chemically pure cyanide of potassium could be obtained.

To reduce the excessively strong deposit of metallic silver into iodide of silver by means of an iodine solution, and subsequent refixing in hyposulphite of soda, is theoretically quite correct, but can be done only with much care and attention by an experienced operator. The method is worthless when the reduction to iodide has been carried on but a trifle beyond the proper point. The hypo bath must necessarily take off more of the silver haloid formed than was intended, the negative becoming, as a consequence, too feeble for printing.

Not much better did we fare with the method of reducing the silver deposit to chloride of silver by means of weak solutions of ferric chloride. With the subsequent refixing in hypo, no more reliable results can be secured with this method than with that of iodide of silver. It is true that the conversion of the metallic deposit into chloride of silver has proved to be very useful in removing green and red fog, or discolorations of the film, excepting those caused by sulphides formed, but before a print can be made from the negative thus transformed into chloride, a redevelopment with ferrous oxalate must be resorted to, which, however, by no means reduces the original density of the plate, but increases it very perceptibly.

Reducing through the transformation of the silver into chloride is risky, for many reasons. To judge approximately correct of the degree of the reduced intensity, before refixing the plate, requires a most experienced eye. An insufficient amount of chloride formed necessitates a repetition of the process, and when it is excessive, the whole proceeding results in a failure and the loss of the negative. If the negative has been primarily perfectly washed, the transformation into chloride of silver progresses uniformly and slowly, but the presence of a mere trace of hyposulphite of soda is an obstacle to success. Ferric salts (ferric chloride) are reduced into ferrous by hyposulphurous salts, and if the negative has not been totally freed from the fixing agent, reduction into chloride can take place but imperfectly, or not at all.

Reducing agents containing principally iron chloride and similar substances have been frequently proposed for practical use, and are, in fact, prepared and sold with great praise of their virtues. Apparently these media act well, slowly, and uniformly enough to allow of a perfect control and with satisfaction to the casual observer. The chloride of silver formed, however, remains in the film, as it is not removed by fixing, as in the previous cases. What will become of a negative reduced in that way, and if a large edition is to be printed from it, I leave to every printer to decide.

Cupric chloride, formed when cupric sulphate and sodium chloride is applied to the negative, is just about as unreliable as any of those previously mentioned. The action is based upon the same principles. Owing to the copper salt, the deposit formed assumes a non-actinic color, and unless the silver chloride is fixed away, and not even then, can great success be expected from the process.

Dr. Eder has constructed a negative reducer consisting of cyanide of potassium, iodide of potassium, and mercuric chloride. Its action is similar to those we have considered, but an excess of cyanide of potassium dissolves the respective chloride, cyanide, and iodide of silver formed, thus effecting the desired reduction of the negative deposit. Eder's solution is easily controlled in its action, but the reduction takes place slowly, an hour or even longer being requisite to reach the desired effect. Hence this process, with all its advantages, can never become quite popular in a very busy laboratory.

Farmer's solution, consisting of a mixture of ferri-cyanide of potassium and hyposulphite of soda, is probably the most generally accepted method to reduce over-intensity in negative deposits, as well as in printed paper proofs. It is controllable and safe, leaves no color stains behind, and acts with such an energy that, after a short application, the whole silver deposit may be totally destroyed, leaving upon the glass a pure gelatine film without the least trace of an image. When ferri-cyanide of potassium, itself sensitive to light, is mixed with hyposulphite of soda and then exposed to diffused light, only for a short time, the mixture will turn blue, and, owing to a separation of sulphur, turn milky. A part of the ferric salt is reduced to ferrous, ferri-hydrocyanic acid formed, sulphur, Prussian blue, and probably hydroxide of iron separated.

Under the influence of light Farmer's solution will soon become inactive, and should, therefore, be applied only in the dark room, or in a much subdued light. With proper precautions the solution has proved to be extremely active, and, as its energy can be well controlled with the greater or less amount of the ferri-cyanide of potassium used, it has been almost universally adopted. The process is truly chemical; the ferri-cyanide of silver forming is dissolved in the hyposulphite of soda.

As a reducer for negatives it is effective when the general density of the plates requires modification, or when only a local reduction is desirable. The solution, mixed with strong gum arabic mucilage, and applied with a soft brush to the parts to be reduced, gives, with a little care and dexterity, all desirable effects. Halation marks, excessively developed whites, and even flare spots or ghosts have been removed or subdued by it. For the reduction of paper prints it is best to soak them in hypo solution, and add gradually to it but small portions of the ferric salt.

Less successful has been its application to the Eastman permanent bromide paper, which, more or less, assumes a yellow tone with it, the method now to be spoken of giving more satisfactory results.

Not less effective than Farmer's solution, and preferable to it in many respects, is the method with potassio-ferric oxalate, proposed by Monkhoven, introduced and made practical by Bellitski. Its peculiar virtues are that even by long-continued action no trace of color is left behind, therefore is more useful in reducing gelatine paper prints, like those upon the bromide paper. It acts probably slower but more steadily than Farmer's solution, but as the action begins only when the entire film has been penetrated by it, reduction must not be carried to exactly the degree of intensity required, but somewhat below it; the solution continuing to act within the film for some time after the operation has been interrupted by washing the plate. Potassio-ferric oxalate is the beautifully green crystallized double salt of ferric oxalate and oxalate of potassium which is formed by higher oxidizing our ordinary ferrous oxalate developer.

If this highly energetic developer is exposed to air but a short time, the active ferrous salt takes up more oxygen, becomes ferric, and ceases to be a developer. Hence old or partly oxidized developer is quite frequently used as a restrainer, or to commence the development of plates suspected of over-exposure. By higher oxidation the oxalate developer loses part of its energy (ferric oxalate does not develop). If, therefore, the ferrous salt remaining in the solution in lower quantity does the work, would it not be as well to apply for this purpose a freshly prepared ferrous solution containing an amount of the iron salt equivalent to that existing in the oxidized solution? But this does not seem to be the case. A peculiar virtue is displayed by the salt of higher oxidation; it possesses a restraining power by which most excessive over-exposures can be well controlled. The ferric oxalate does not develop, it restrains, and will ultimately, under certain conditions, reduce the metallic silver deposit on plates developed with any of the known developers.

Before we consider the action of this reducer, it may be as well to describe the sources from which the valuable and beautiful salt is obtained, the more so as many practitioners have been prevented from using it on account of its scarcity, or by being ignorant of its mode of preparation. Old oxalate developer exposed to air in open vessels will, after a short time, deposit potassio-ferric oxalate in the form of green crystals; it will do quite well to separate them from the solution, remove the brown iron oxide adhering by washing in water, dry the crystals between bibulous paper, and preserve them for use. But if it is desirable to recover the whole quantity of the salt, proceed as follows: Take the whole amount of old developer on hand, pourit in an evaporating dish, and allow it to evaporate spontaneously in a subdued light. The resulting brown, powdery mass, interspersed with a multitude of green crystals of various size, is then to be mixed with one-tenth or one-fifteenth of its weight with crystallized oxalic acid, and with five or six times its volume of water, boiled in an evaporating dish. The resulting thick and muddy liquid is filtered into another evaporating dish while yet hot, the

* Read at the Minneapolis Convention, July, 1888.

dish covered up with paper, and set aside in a cool place overnight. From it form beautiful crystals of a pure emerald green color—the potassio-ferric oxalate.

The salt is sensitive, light reduces it again to ferrous oxalate; it must, therefore, be kept in the dark. The salt can also be prepared by adding to a weak solution of ferric chloride another weak solution of neutral oxalate of potassium, filtering the solution, and crystallization. It is necessary to add enough oxalate to secure perfect neutralization. An excess of it will soon precipitate; an excess of iron chloride renders the resulting salt unfit for use.

The formula for reducing is simply to take ten parts by weight of the green crystals, previously dissolved in as little water as possible, and add it to one hundred parts of the ordinary hypo solution. When an over-intense negative is subjected to this compound, the reduction will take place slowly but perceptibly, and the process can, therefore, be easily controlled. The hypo-sulphite plays an important part. Without it the potassio-ferric oxalate does not reduce. A negative perfectly free from hypo may rest in a pure solution of the green salt indefinitely without any apparent effect. Remove the negative to a very weak solution of hypo, and reduction will commence at once. Hence, it will be apparent that washing after fixing is not at all necessary when the negative is to be reduced.

The chemical process taking place has not been definitely stated, but the method is so secure, controllable, and in no case discoloring the plate, that it may well be considered of much practical value, more valuable, perhaps, than any other reducer.

I must not omit to mention, in connection with the chemical reducers which we have considered, one whose action is merely mechanical. I refer to the method of rubbing down excessive intensity by means of a linen rag moistened with alcohol. Friction reduces density by taking off particles of the silver deposit. The intelligent negative retoucher can by this means produce very fine effects, and make visible modulation quite obscured upon the crude negative.

ELECTRIC LIGHTING IN AMERICA.*

By Professor GEORGE FORBES, F.R.S.

HAVING since the last meeting of the British Association had occasion to pay two visits to the United States, and while there to study very carefully the systems of central station lighting in vogue, and also to compare what was being done there with what was being done here, and being, further, able to compare the state of central station lighting in America as it is at present with the condition it was in four years ago, when this association visited America, I felt it was a sort of duty I owed to the association to bring before it some account of what I had seen, believing it would be especially interesting at the present moment, when there are signs of a very great determination on the part of the public in this country to have electric light supplied to them from central stations. At the same time I felt considerable diffidence in bringing before you the facts which have been collected—not in the least because there is any want of facts, but by reason of their number, which renders it difficult to make a proper selection of the points likely to be of interest to the members of this action.

In the first place, it has been often stated—and one cannot help commencing remarks of this kind by repeating—that enormous progress has been made in central station lighting in America. At the moment one lands one is struck by it; and when one lands, upon returning to England, one is equally struck by the dimness of the lighting in our streets. In America there is not a single town with which I am acquainted where you can walk in the streets at night without seeing very many arc lamps. I am not simply speaking of influential towns like New York, Philadelphia, and Boston, but of the most out of the way places in the most distant States, where gas has never penetrated. There electric lighting has fixed its position, and it is being used frequently. Not only has the electric light been adopted in these remote towns, but other developments of electricity have followed. As an illustration of the kind of places to which electricity and electric light have found their way, I may mention that I have a paper here published in a village in Alaska, one of the least developed of the Territories of the United States, and the same number of this paper which treats of the introduction of electricity in a scientific manner treats also of the advisability of passing a law for the expulsion of Indians from the city at nine o'clock every evening.

At the time I was attending the electric light convention at Pittsburg, last February, it was estimated that there were then 300,000 arc lamps lighted from central stations in America. The progress of arc lighting of late years, however, has not been so striking to an Englishman as formerly; the arc light was firmly established when the British Association visited America four years ago. The progress which has struck us all latterly has been the progress in incandescent lighting. At the same time—I am speaking of February of the present year—it was estimated that there were 2¼ millions of incandescent lamps in use in the United States; and the rate of manufacture has increased enormously, to my knowledge, since that short time ago. There are four or five manufacturing places in the United States at this moment, each capable of turning out 10,000 incandescent lamps every day, and I have no doubt that the annual production at this moment is to be counted by millions. Of incandescent central stations there are two classes, one of which is the three-wire system, in which a continuous current is supplied to the main conductors over a town and the lamps are connected by branches to the system of positive, negative, and intermediate mains. That is the system which is so largely adopted in all the Edison stations. It was the Edison stations which interested us most in 1884, and they have continued to advance and have developed the three-wire system to a very great extent indeed.

But I feel quite sure that the greatest progress that has been made in central station lighting in America since the Association visited it has been by the work which Mr. Westinghouse has done in that country. One of Mr. Westinghouse's agents in passing through Europe saw the experiments which had been made by Messrs. Gaulard and Gibbs in this country in the use of alternating currents to distribute the electric light; by means of this system an alternating current of very high tension could be passed along the streets over conductors of very thin diameter and very cheap to construct, and at the consumer's premises this high tension dangerous current could be reduced in tension by means of an induction apparatus called a converter, and passed into the house at a harmless pressure, suitable for the ordinary incandescent lamps.

And here I may say that I never fail to take any opportunity which presents itself to me to say what I feel with regard to the energy and determination with which Messrs. Gaulard and Gibbs, in spite of the most adverse criticism which was universally passed upon their first attempts, persevered and continued to improve their invention, and eventually proved to the world that it was possible to have a commercial system by means of these converters.

Mr. Westinghouse, on seeing this system, took it up, and the result is that he has established, in the course of a very few years, an enormous business in central station work. It is now a possibility in America—not a possibility merely, but a thing of daily occurrence—for a town to write an order for a central station, which has to be sent out at ten days' notice. They do not order a dynamo, set of conductors, and other apparatus all independently, but complete central stations are being sent out at a steady and uniform rate. In December of last year the manufacturers of incandescent lamps drew out a long list of the number of lamps being used in different parts of the country, and I have abstracted from that list some notes which show that at that time Mr. Westinghouse's alternating current system was supplying 153,285 lamps, occupying 152 distinct plants, and 42 of these had plants for more than 1,000 lamps. At the present moment they have set up 110 central stations, with 191,000 lamps.

It has been said that in this country we do not go ahead so much as they do in America, and various reasons have been suggested to account for this. Professor Ayrton's most admirable reason seems to go a long way—namely, that there are two definitions of inertia, the English, which is that inertia is a resistance to motion, and the American, that it is a resistance to stopping. But I must say that in electric lighting we certainly have been stopped a great deal by what I might almost call the iniquitous electric lighting act. And I regret extremely, in the interests of the public and of electric lighting generally, that an act has been passed this year which has not yet put electricity on the same footing as gas. This is a matter for extreme regret to all those who have the ultimate benefit of the electric lighting industry at heart, for that act will delay the due recognition of the value of electric lighting.

But I must say that I do not think the electric lighting act is the only thing which has delayed our progress, because I see that in electric traction and other developments of electricity also the Americans have gone ahead with great energy; and I certainly do not put it down to want of capacity on the part of our inventors, for I find that the best dynamo machines are all designed originally in England, and the best arc lamps also are to be found in England; but with inferior machines and lamps in the States, I have noticed that they are determined, as long as they have something which they know will work well, to make it work, and be ready in the meanwhile to take up something else. I think, therefore, that it is not in the inventors that we have to look for goaheadness, but to men of capital who are technically instructed. I think it is technical instruction among the moneyed classes that has developed electrical engineering so much in America of late years.

I will now try to describe some of the leading features of the Westinghouse central stations which I visited, and perhaps this may be of use in helping us to draw some conclusions as to the lines in which we are to develop electric lighting in our own country.

I do not in the least wish to say that the American arrangements are the best. In many points I see objections, and many where I should wish to see improvements; but we have in the system which I am going to describe a thoroughly successful working arrangement, and although we in this country may not adopt all the points of such an arrangement, still I feel that it will be of use to us to realize what is being done on the other side. It is a curious thing, for example, that in America there is no work hardly being done with accumulators. There is an example where we have led the way, as compared with the Americans. I was struck with the fact in 1884, and still more in the present year, that accumulators do not seem to work in the American atmosphere; at any rate, people have been importing numbers of accumulators, and yet they are not recognized as the satisfactory apparatus we have come to regard them as; they are looked upon there as being as uncertain now as they were with us four or five years ago.

The Westinghouse system, then, chiefly depends upon the apparatus which has been called by the names secondary generator, transformer, or converter; "converter" is the term generally adopted in America. This is simply an induction apparatus with two coils, a primary and a secondary. The primary coil, in contradistinction to the ordinary Ruhmkorff induction coil that we have known so long, is of high resistance, with a large number of turns on the primary and a small number on the secondary, which goes to the lamps. The alternating current produces induction in passing through the primary, which reacts on the secondary, thereby producing a current of low tension going into the houses. The primary coil and the secondary are completely separated, and when properly constructed it is quite impossible that the high tension current can ever reach the secondary circuit, so that the consumers of electricity have nothing to fear from the dangerous current which is distributed from the central station.

A central station contains furnaces and boilers, engines, alternate current dynamos, exciting dynamos, switch boards, testing instruments, repairing shop, and stores. The scheme of arrangement of the mechanical and electrical parts is generally the same in every station.

The wires led out from the station are carried generally on poles as feeders to the separate districts. The converters are generally attached to the outside wall of the house to be supplied; but they are often attached to the poles carrying the feeders. They are sometimes placed inside the houses. The secondary wires inside the house, to which the lamps are attached, are laid in the usual manner, and are connected to the secondary poles of the converter. The dynamos are run day and night without cessation; charges are made by contract or by meter.

We hear a good deal here of the impossibility of running machinery day and night without breakdowns, and I cannot help rather indorsing the opinion I heard universally expressed in America—that this fear has been enormously exaggerated in this country. When we see our huge 10,000 to 15,000 horse power engines crossing the ocean, performing their work for days and weeks without breakdown of any sort, it seems absurd to expect that where we are able to subdivide our power, as in a central station, there should be any serious breakdown in a properly organized system. As a matter of fact, I assure you that in America plants which have been run without accumulators have been going on for years and years, and it is a matter of the utmost rarity for an accident to arise of sufficient importance to stop the general lighting of the district. The Edison central station in Pearl Street, New York, which has been running many years, has, I believe, only once broken down, and that was an accidental occurrence due to too much current being turned on on a foggy day, and the experience of other central stations is to the same effect. The general view in America is that there is no danger of a breakdown ever happening in a properly organized station.

It happens that in the department of furnaces and boilers there is much of special interest in many of the Westinghouse stations. This arises from the fact that over a great part of Western Pennsylvania the fuel employed is the natural gas obtained from the wells in that region, and carried in pipes to considerable distances. But this is a feature so peculiar to the district that it requires only a short notice here.

The gas, with its due proportion of air to produce the high temperature of a Bunsen burner, is admitted by pipes with a sufficient number of nozzles into the combustion chamber. Here the burning gas passes over a mass of bricks with ventilating spaces, and thence through the tubes of the boilers. The gas is nearly free from carbon, and the hydrogen, which is paramount, undoubtedly weakens the iron of the boilers; but practice shows that this action does not go far, and the iron acquires a final condition in which it does not deteriorate, but seems capable of indefinite use.

In the best stations the admission of gas and air to the furnaces is regulated by means of a valve controlled by a lever, which in its turn is subject to the pressure of the steam in the boiler. This automatic regulation is very perfect, so that the attendance of one man to the boilers and furnaces of a station with several thousand horse power is quite sufficient.

As to steam engines, there is no need to say much, for I do not think the Americans can teach us anything about making steam engines. But there are some points in connection with the class of steam engine used which I think are of importance. A great deal has been said about the desirability of having central stations furnished with large, powerful, slow-running engines which are economical in the consumption of coal. Practical experience, however, has decided the question for the present exactly in the opposite manner. It is found to be more economical and safer to have a number of dynamos in each station, each driven by a separate engine of high speed, the power being transmitted through a belt, and with no countershafting.

About the rights of this question there cannot be the slightest doubt. It has been proved over and over again by experience. At first sight it does not seem probable that a number of independent high speed engines should be more economical or safer than a single large engine, say of the marine type. The explanation lies in the special conditions of central station lighting. Without dwelling upon the great loss of power due to countershafting with the large engine, it is only necessary to observe that the maximum load in a lighting station is many times as great as the average load. Thus, if the single engine be adapted for the maximum load, it is working underloaded during the greater part of the twenty-four hours. The consumption of steam in proportion to power developed is very high, and the engine is, under these circumstances, not an economical one. With the independent engines, on the other hand, none of them need at any time be working much under its best load, and when the load increases other engines and dynamos can be switched in, and they are thus always working at or near the most economical conditions.

It has been found from actual practice in electric light work in identical central stations that 20 per cent. more work is required in the case when countershafting is introduced.

It has also been proved, from the experience of numerous central stations, that during a considerable number of hours in the night the load on the engines and dynamos is only one-eighth part of the maximum load.

For these reasons it is found most economical to use with each dynamo an engine whose most economical load is something like 20 per cent. under the maximum load. Waste of steam is far greater with underloading than with overloading.

Another advantage of this system is that there is abundance of time for the inspection of engines and dynamos. The station is easily added to. A breakdown in an engine or dynamo does not stop the working of the station. There is one other factor of safety to which attention must be drawn. One of the most annoying accidents in an electric lighting plant is that due to a short circuit on the mains. In overhead work this often happens by the falling of broken telephone or dead wires upon the mains. In this case a small independent engine is pulled up and no damage is done. With the large engine the dynamo grinds on, and unless the cause of short circuit is burnt up, the dynamo is destroyed, or at any rate the whole of the working is put out of gear.

Now I have a word or two to say about dynamos and converters, the actual apparatus which are used in America. I attribute a great deal of the manufacturing success of American electric lighting plant to this, that they fix upon the types of machine that they are going to make, and then lay down machinery which will enable them to produce those types cheaply, and having thought out the system thoroughly in the beginning, they do not require to make a completely new type of machine for every order that comes in. They are then able to manufacture their machinery cheaply, and in such a way that every part is replaceable.

* A paper read before the British Association, Bath, 1888.

At this moment, I think if I had to choose between two dynamos, of which one was a little better than the other in efficiency, but the other of which was manufactured in this way, I should be very much inclined to lean in favor of the one which was made by machinery, and of which all the parts were interchangeable, and of which I could get any one of the parts by return train by telegraphing for it. The convenience of such a machine is obviously great.

I have, for the benefit of those who are electrical engineers and manufacturers, thought it might be of interest to put down some facts in connection with the three types of alternating current dynamo machines made by the Westinghouse Company. There are three types, one for 650 lights, another for 1,300, and another for 2,600. The voltage of all is the same, 1,050. In this country there has been a tendency to use on the primary circuit a higher tension than that, 2,000 volts. In the Westinghouse system they use 1,000 volts at the converters, and 1,050 is developed by the machines. The local pressure is reduced in the same proportion as has been done most generally in Europe, that is to say, in the ratio of 20 to 1. The 1,000 volts in America is reduced down to 50 volts in the lamps, and in this country the 2,000 volts has been reduced to 100 volts at the lamps. And here there is a question opened which must be decided immediately in the central stations which are going to be put down in this country, whether you are going to take the full advantage, as Mr. Westinghouse does, of being able to carry your high-tension current in the mains.

The advantage is that you are able to save copper to a very great extent indeed, whether you use 1,000 or 2,000 volts, and then, as Mr. Westinghouse argues, we are able to use lamps of only 50 volts instead of lamps of 100 volts; that he looks upon as the greatest advantage he has gained by the introduction of the Gaulard and Gibbs system. We all know that the higher the voltage of the lamp the shorter its life, and a 50 volt lamp is a far more perfect thing than a 100 volt lamp. We are greatly troubled by the expense of lamps compared with their life. That is an expensive item in the supply of electricity, and a 50 volt lamp being far more economical, is it not desirable to follow the American plan and use the lower voltage? I am sure that electrical engineers generally will see that there is a point worthy of their serious consideration in this question.

It may even be said that we are all to go still further and reduce the voltage a great deal more. Of that I am less certain; but there is another point which will occur to electrical engineers, whether it is not desirable to use the lamps which Mr. Bernstein has been perfecting, of very low voltage, placed in series. At present one of the most serious difficulties in introducing electric light for domestic purposes from central stations is in the cost of wiring houses. Now, if we place our electric lamps all in series, the cost of wiring will be very much diminished, and it becomes a question whether this is not the best arrangement. In that case the primary coils of our converters would be put in series in the same way as they were originally in the Gaulard and Gibbs system. We shall then be doing what they were striving to do when they chose the series arrangement, namely, not by having the two primary terminals inside the house at a difference of potential at 1,000 or 2,000 volts, which would be dangerous, but no two conductors of greater difference of potential than about 50 or 100 volts. If at the same time we use low voltage lamps, we shall get a longer life than even Mr. Westinghouse has obtained. It seems to me that there are grave reasons for considering most seriously the question of arranging all our lamps in series and the primary coils in our converters in series also.

But I am not here to put forward suggestions about what we may probably do in England, but only to tell you what they are doing in America, wishing at the same time to guard myself from saying that they have the best arrangements in every possible point.

The construction of the Westinghouse dynamo is pretty well known in this country, and it is needless to speak of it.

I think we are probably all agreed that the old type of alternating current dynamo, such as the Siemens, has probably seen the end of its days. One of the chief difficulties seems to be that when you have such a large number of coils revolving between the poles you are obliged to make a considerable clearance, and that causes you to use up a great deal of power in magnetizing the field magnets. Mr. Westinghouse makes a very small clearance, and in that way uses up only 2 per cent. of the maximum power.

The Westinghouse Company make those types of dynamo, of which the following particulars are of interest:

Dynamo No.	I.	II.	III.
E. M. F.	1050	1050	1050
Current	35	65	130
Resistance (armature) at 30° C.	0·76	0·37	0·15
" (fields) " " "	14·5	7·0	3·6
Weight of wire in armature	17	30	60
" " fields	420	.....	.....
Total weight	4800	9000	.....
No. of lights	650	1300	2600

The No. III. has an armature about 2 feet diameter and 2 feet long. It has 16 poles, and runs at 1,000 revolutions per minute. The armature plates have each six large holes for ventilation and lightness. The weight of armature is 2,000 lb. The insulation is mica and copal varnish, which is found to be infinitely superior to shellac or any other material used.

The converters used by the Westinghouse Company are of five sizes, called by the numbers I., II., IV., VI., and VIII. The following table gives particulars about them. The size of wire is on the B and S gauge:

Converter	I.	II.	IV.	VI.	VIII.
No. of lights	5	10	20	30	40
Wire on primary	25	22	19	17	16
" " secondary	11	8	8	8	7
Turns on primary	900	700	560	480	400
" " secondary	45	35	28	24	20
Resistance of primary	48	21·9	9·9	7	5
" " secondary	0·04	0·043	0·0197	0·0176	0·0107
Weight finished (lb.)	50	60	95	.....	160

Thickness of iron plates = 0·006 to 0·0065 inch.
" paper insulation = 0·0025 inch.
Number of plates in a No. 8 converter = 1,350.
The iron used is Welsh Taggard iron.
50° C. rise in temperature is allowed in the converters.

In this country the tendency is to make converters much larger; it has even been suggested that they should be made of thousands of lights. But for ordinary use in houses the Westinghouse system is the simplest, more especially as we find that it is necessary to wire the houses in such a way that they will suit our meters, which do not at present go over a range of from one lamp up to a thousand lamps, nor even up to hundreds of lamps, and if we find that our range is suitable up to 40 lamps it is very desirable to wire the houses in such a way that there are not more than 40 in the circuit of a meter indicating 40. For this reason Mr. Westinghouse has limited his converters in this way.

There are some points well worthy of attention in regard to converters. In the first place, by the use of only 1,000 volts, a far cheaper converter may be made, and thus a great saving may be effected in the plant required for a house. Then, again, there is the rapidity of alternation. In the Westinghouse plant there are 16,000 alternations, 8,000 complete periods, per minute. In this country the tendency is to make the alternations less than that. The quantity of iron you put in your converters depends on the number of alternations; the greater the number of alternations, the less iron is required, and you are therefore able to use smaller converters. I do not know whether or not there are objections upon other grounds to a high number of alternations. The only objection that occurs to me is that, while the construction of the dynamo sometimes becomes a little easier, it also takes a little more space.

Mr. Westinghouse asked me to make some tests for him on the efficiency of his converters. It has been supposed that converters have not a high efficiency when working at half-load. I may mention that the efficiency varied slightly in the different types, owing to the proportions of iron being greater in some types than in others; but at half-load in some cases we were able to get an efficiency of 95 per cent., which is a splendid result, and I think a greater efficiency than most people have been supposing was to be attained. I do not think that in actual practice there is an average waste of 10 per cent. in the use of converters, when they are properly designed. The formulæ which regulate the construction of converters have been given by me in the *Journal* of the Society of Telegraph Engineers and Electricians, February, 1888.

I may also say that Mr. Westinghouse has in his lamps 70 per cent. of the power given to the belt by his engine, which is a very good return for converters and alternating dynamos, showing that the new alternating current machines are much superior to the old ones, which gave about 70 per cent. efficiency.

The wiring is nearly all overhead in the States. The wire used is called simplex wire and is made by a firm in Boston. It is very carefully prepared, and is, I think, beginning to be known in this country, and gives satisfaction.

For underground work Waring cable, which is lead-covered, is used with complete success. I am informing you now of what is being used, and used successfully, and what may be used successfully by us in this country; but you must not suppose that I hold that these arrangements are the best.

Now comes a most important point of distribution. Ought the conductors to be massed in one heavy main, with all the dynamos feeding it, or ought the main to be much subdivided, with, perhaps, several to each dynamo? or ought we to take an intermediate arrangement? The experience of the Westinghouse Company leads to the subdivision of mains as far as possible, because a short circuit on any main then endangers only one dynamo.

I have often been asked the question whether alternating dynamos can be worked in parallel. They can, when they are fully loaded down almost to half load, but below that they do not synchronize. Great subdivision of the mains has been found in practice to facilitate largely the working of the central station.

There is one thing that will probably be spoken about in the discussion which will follow my remarks, and that is the high speed of the Westinghouse dynamo. I think there is needless objection to high speed by engineers in this country. If we have a uniform cylinder of iron revolving, I have no objection to any rate of speed so long as I know the shafting will not give way.

I think it would be desirable that some of the gentlemen present should give us their views as to the desirability of using a high or low number of alternations, high or low voltage in the primary, whether 50 volts will be desirable in use, and also as to whether subdivision of mains is desirable, and whether the converters should be supplied with only a limited number of lamps, such as 40 or 50.

I have now attempted to give you a general idea of working central stations in America on the system which has been most fully developed of late years. It is the result of a great amount of careful work, and in the work of the immediate future in England I am sure that English engineers will be benefited by the large practical experience which our transatlantic cousins have collected.

COVERED RESERVOIRS.*

By CHAS. H. SWAN, Civil and Sanitary Engineer, Boston, Mass.

ANCIENT COVERED RESERVOIRS.

COVERED reservoirs have been used for the storage of water from periods of great antiquity. They have been constructed of various sizes and shapes, from the small cisterns supplying single dwellings to the large reservoirs of ancient fortified cities and of modern municipal water works.

During recent excavations at Jerusalem,† many ancient reservoirs have been discovered. Those which appear to be the oldest, and of very great antiquity, were formed by sinking deep wells through the rock and then making an enlargement at the bottom to act as a collector. Second in antiquity are the cisterns with natural roofs. These were excavated in a stratum of softer rock, the overlying harder stratum serving as a roof. A third class was formed by excavating the rock and covering the opening with an arch. A fourth class, modern, were built amid the loose debris, the accumulations of the centuries, which forms a large portion of the site of the modern city. It is estimated that the ancient subterranean cisterns in the vicinity of the Temple alone contained upward of 10,000,000 gallons. One of them, known as the Great Sea, is said to have had a capacity of 2,000,000 gallons. Numerous other cisterns, some of them of large capacity, have been discovered, excavated in the rocky hills of the vicinity.

Although foreign to the immediate purpose of this paper, it may be interesting to mention a few facts relative to the ancient water supply of the city. Besides the water furnished by springs and wells, and by the collection of rain water, there was a large supply brought from a distance by aqueducts. The remains of two such aqueducts are in existence; one the high level, the other the low level. The latter was over 40 miles in length, and still carries water as far as Bethlehem. It corresponds in length with an aqueduct known to have been built by Pontius Pilate, but it cannot be fully identified. The high level aqueduct drew its supply from a tunnel four miles long, excavated in the rock beneath the bed of a valley on the road to Hebron. Near Rachel's tomb it crosses a valley by means of an inverted siphon, 15 inches in diameter, formed of large perforated blocks of stone set in a mass of rubble masonry. The joints seem to have been ground. They are united with an extremely hard cement. This aqueduct cannot be traced within some miles of the city, but it is supposed to have entered near the northwest corner and to have supplied the upper part of the city.

These brief statements, which are from the published accounts of the work of the Palestine Exploration Fund, show, as do the remains of Roman water works at Lyons, France, that the principles of hydrostatics were known to the ancients, who were prevented from applying these principles extensively by the difficulty of obtaining, in those days, pipes of sufficient size and strength to convey large volumes of water under heavy pressure. The familiar arrangement of separate aqueducts under different heads for supplying high and low districts is also seen to date from a very remote period.

Many fortified cities of antiquity were provided with subterranean reservoirs to supply water during sieges. The ancient cisterns of Constantinople are reported to have contained a supply for 1,000,000 men during four months. Several of them are still in existence. Other ancient covered reservoirs might be mentioned, but these examples are sufficient to indicate their magnitude and importance.

MODERN COVERED RESERVOIRS.

The covered reservoirs of modern water works owe their origin to the necessity for maintaining the purity of the supply when it is stored in the vicinity of large cities. The effect of covering is:

1. To protect the water from solar heat and light; thereby securing uniformity of temperature and preventing the growth of vegetation.
2. To protect the water from atmospheric impurities.
3. To prevent malicious pollution.

These three classes of evils vary in relative importance in different climates and localities.

EFFECTS OF LIGHT AND HEAT.

It has for many years been recognized that water derived from certain subterranean sources is peculiarly liable to be invaded by vegetable growths upon exposure to the light and heat of the sun in open reservoirs. Examples of this action have been found in water from various geological formations, both in Europe and in the United States. Apparently, such water contains principles which do not affect its clearness or limpidity, but which promote vegetation upon exposure to the light, rendering the water unsightly, and frequently developing a disagreeable taste and odor. The examinations of the water supplies of towns now being made by the Massachusetts State Board of Health will, it is hoped, furnish an explanation of this curious fact.

An interesting illustration of the effect of excluding sunlight from a water derived from subterranean sources, and which had caused complaint, is given by Mr. G. H. Parker in a recent report,* some of the details of which are as follows:

The town of Brookline is supplied with water from a covered filter gallery. The water is pumped into two reservoirs, one of which is an iron tank supplying the high service. Complaint having been made that the water supplied from these reservoirs had an offensive taste and smell, while the water in the filter gallery, which was in darkness, remained free from any disagreeable qualities, it was deemed advisable to cover the high service tank with a double roof, and ascertain whether the exclusion of light would prevent the development of the unpleasant changes.

Examinations made subsequent to the covering of the tank showed that while the water in the filtering gallery continued to be clear, free from green algæ, and devoid of disagreeable taste or smell, and while the water from the open reservoir continued to show "an abundant supply of green algæ, was slightly cloudy, and had a very strong taste and decidedly fishy smell," the water from the high service tank, "now completely darkened, contained only one specimen of green alga, was now free from odor and taste, and for all practicable purposes as good as that pumped at the filter gallery." It was concluded that the exclusion of light would be a complete remedy for the unpleasant effects, and the construction of a covered reservoir is contemplated.

Similar conclusions have been reached in England. Thomas Hawksley, the celebrated English engineer, testified in 1852 before the select committee on the metropolis water supply bill, that water taken from the new red sandstone was peculiarly liable to the growth of algæ if it was exposed to the sun, but that the exclusion of light and heat was a complete preventive. He

* From the Journal of the New England Water Works Association, Sept., 1888.

† The Recovery of Jerusalem. New York, 1871.

* Massachusetts State Board of Health. Nineteenth annual report. Boston, 1888.

also said that he had recommended covering a reservoir at Liverpool, a short time before, to avoid these unpleasant results.

The water of lakes and rivers may also be invaded by growths of algæ under favorable conditions. This growth is greatest in shallow, open reservoirs or ponds, where the water is comparatively still, and where it is exposed to a considerable elevation in temperature. In reservoirs exceeding 12 or 15 ft. in depth the growth does not appear to be so troublesome.

ATMOSPHERIC IMPURITIES.

The danger of pollution from atmospheric impurities is greatest in the neighborhood of smoky cities. At London this was formerly the source of great annoyance. The surface of the water in reservoirs was often covered with a film of soot and dust, and if the water remained in the reservoir a sufficient time, it acquired a bitter flavor.

The general board of health, in its report in 1852 on the supply of water to the metropolis, in consideration of this trouble and also of the fact that filtered water after exposure in open reservoirs frequently had to be strained before it could be used for domestic purposes, on account of growths of algæ in the reservoirs, arrived at the following conclusion: "Against the modern engineering practice of exposed and open reservoirs we would rather revert to the custom of the Roman engineers, and recommend covering the service reservoirs and aqueducts to the utmost extent practicable."

Influenced by similar considerations, the select committee of the House of Commons reported a bill, which was passed July 1, 1852, containing the following clauses: "Every reservoir within a distance in a straight line from St. Paul's cathedral, in the city of London, of not more than five miles, in which water for the supply for domestic use of the metropolis or any part thereof is stored or kept by any company, shall be roofed in or otherwise covered over: provided always, that this provision shall not extend to any reservoir the water from which is subjected by the company to efficient filtration after it is discharged from such reservoir and before it is passed into the mains or pipes of the company for distribution, or to any reservoir the whole of the water from which is distributed through distinct mains or pipes for other than domestic purposes, nor to any reservoir whatever the water stored in which shall be used exclusively for other than domestic purposes. . . . No water shall be brought or conducted within the metropolis by any company for the purpose of domestic use otherwise than through pipes or through covered aqueducts, unless the same shall be afterward filtered before distribution."

English engineers, having in mind the conditions obtaining in England, appear to regard protection against soot and dust as the most important point, although they by no means ignore the importance of protection against heat and light.

French engineers, on the other hand, seem to consider protection against solar influences as the most important. This naturally follows from the fact that French cities are much less smoky than English cities.

The atmospheric conditions in the United States resemble those of France, although there are localities in this country which are as smoky as any in England. The need of covered reservoirs has been felt here for several years, especially since the use of ground water as a supply, and the practice of filtering, have become common, and several have been constructed.

(To be continued.)

ON VARIABLE LOAD, INTERNAL FRICTION, AND ENGINE SPEED AND WORK.*

By ROBERT H. THURSTON, Ithaca, New York, Member of the Society.

IN a recent paper on the "Internal Friction of Non-condensing Engines," the writer gave the result of a series of experiments made to determine by further research the character of the internal friction of engines without condensers, and the method of its variation with variation of the usual conditions of operation. It had been shown previously that, in some classes of engine at least, this friction is constant with all loads whatever, up to and beyond the rated power of the machine. It was, in the last paper,† incidentally stated that the experiments kindly directed by Professor R. C. Carpenter, in the laboratory of the Sibley College of Cornell University, has indicated a slight increase of internal resistances with increase of engine speed. For the small engine then tried, this increase amounted to eight per cent. of the number of revolutions made by the engine per minute. It is thus found, for that case, that the percentage of power lost by friction was a constant fraction of a given total power of the engine at all speeds.

This investigation has been continued during the past college year, and further data obtained from a number of engines of various types and sizes, the results of which investigation are now for the first time herewith presented. A summary of the work on the first engine used, and already reported upon, is here again given for comparison with the later results of trials made with other engines.‡ It will be seen that the method of variation of this friction with change of speed is apparently very largely dependent upon the method of lubrication and its efficiency.

A very similar variation in this respect is to be traced in almost all cases of experiment made to determine the friction of journals or other rubbing surfaces, a very large number of examples of which will be found recorded in the recent work of the writer on "Friction and Lost Work in Machinery and Mill Work."§

One of the engines employed in this later investigation was built by the Lansing Iron Works, of Lansing, Michigan, an engine having a steam cylinder eight inches in diameter, and a stroke of piston of twelve inches. It was fitted with an "automatic gear," and was of the same class and very similar in many respects to the well known Buckeye engine, built at Salem, Ohio. The valve is balanced, and has an unusually quick and wide opening, giving steam in a very satisfactory manner. The alteration of speed was effected by changing the position of the balls of its governor. The brake worked well throughout the tests, and the counting, done by a hand counter, was thoroughly reliable.

The following table exhibits the results obtained, both from this engine and from a 12 by 18 inch engine also employed in the same research. The smaller engine had been in use about a year; the larger was new and had not left the shop.

TABLE I.

FRICTION WITH CHANGE OF SPEED.

Lansing Iron Works. 8 × 12 Automatic.

No. of revolutions.	Average Friction H. P.	No. of trials.
300	2·91	1
220	2·63	6
235	2·44	1
260	2·39	1
175	9·07	2
180	8·87	5
185	10·60	2
190	7·55	4
193	8 11	6
187	9·74	2

Another series of trials was made by the same observers, using a "tandem compound engine," also built by the Lansing Iron Works, having cylinders 14 and 21 in. in diameter, and 20 in. stroke of piston. The two pistons were secured to one rod, and the cylinders were thus placed the one behind the other. Its usual speed was about 200 revolutions per minute, and it was non-condensing. The machine was new and its friction therefore probably greater than it would have been later, by a considerable amount. The data obtained gave the following results:

TABLE II.

FRICTION WITH CHANGE OF SPEED.

Tandem Compound Engine. Cylinders 14×20 *and* 21×20.

Rev. of engine.	Average F. H. P.	No. of tests.
130	21·84	1
136	25·97	1
156	28·49	3
158	28·50	2
159	24·16	2
160	27·78	3
162	25·37	1
165	27·78	3
168	28·35	2
183	30 85	1

NOTE.—The above test shows an increase of friction nearly proportional to the increase of speed, the equation of the curve would be

$$y = 0{\cdot}168\,x.$$

y = number of revolutions.
x = friction horse power.

The most extended range of speed was obtained with a small engine recently brought out by the Lansing Iron Works, and called, from its inventor, the "Jarvis engine." (Fig. 1.) It was one of the first of its class

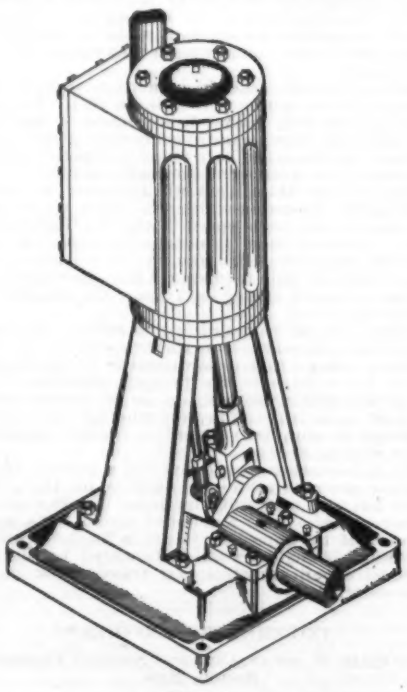

FIG. 1.

and a new engine. In this machine the piston is fitted with a globe joint, by means of which it is attached to the rod, which latter thus vibrates as it drives the crank by a direct connection with its pin. To permit this vibration, instead of having a fixed trunk secured on the piston as is usual, forming a "half trunk" engine, the front head is designed with a globe joint also, in which works a coned sleeve, within which the rod slides, and the whole swings backward and forward as the crank turns and the piston traverses the cylinder. This makes a novel but very practical arrangement, and, so far as experience yet indicates, a perfectly successful one. It has been found possible to drive this engine up to enormously high speeds, and to secure a smooth and safe motion. The piston rod stuffing box is placed at the end of the taper sleeve. This engine has been in operation for six months at the speed of 350 revolutions per minute, making millions of turns, without attention or visible wear. The valve was balanced, and the speed was regulated by a throttling governor. The friction was low, as might have been anticipated from the lightness of the running parts.

The results obtained are shown in the following table. They are substantially the same as with the other engines, the speed variations showing increase of friction with velocity while the engine was underloaded; but it will be seen that the friction later became constant, and remained so, substantially, through a wide range of loading, up to the maximum reached. From 175 to 500 revolutions per minute, the friction increased according to the usual law; but from 500 up to 600 revolutions, the internal friction remained sensibly constant, the loads being light throughout; while, when fully loaded, the speed ranging from about 175 to 912 revolutions per minute, the friction remained very nearly constant, and its variations were irregular. This is well shown by the diagrams which follow this paper.

TABLE III.

THE JARVIS ENGINE 7″×7″.

Friction with change of speed.

Steam pressure constant at 80 lb. Engine running light. Trial No. 1.

No. of Card.	Spring of Indicator.	Revs. of Engine.	M. E. P. Head.	M. E. P. Crank.	I. H. P. Head.	I. H. P. Crank.	Fric'n H. P.
1	20	187	2·2	2·9	0 28	0·375	0·637
2	"	187	2·3	2·5	0·293	0·308	0·601
3	"	168	2·8	2·5	0·329	0·280	0·609
4	"	293	1·8	2·0	0·359	0·427	0·786
5	"	293	1·8	2·0	0·359	0·388	0·747
6	"	293	2·2	2·2	0·440	0 428	0·868
7	40	364	1·8	2·2	0·450	0·532	0·982
8	"	380	2·8	1·8	0·724	0·457	1·175
9	"	375	1·8	1·8	0·459	0·461	0·918
10	"	519	1·7	1·5	0·600	0·514	1·114
11		—	—	—	—	—	—
12	"	554	3·2	2·6	1·20	0·95	2·156
13		—	—	—	—	—	—
14	"	554	2·4	3·2	0·90	1·17	2 07
15	"	633	3·4	3·8	1·46	1·58	3 05

The earlier work already reported was mainly on a single class of engines, and it remained a question whether the variation of load in other forms of engine might not cause variation of the internal friction to an important extent, and according to fixed or various law, and, if so, what were the conditions producing such variations and what were those laws. Advantage was taken of the opportunity presented in this series of experiments upon the Lansing engines to determine what was the variation, if any, of the internal friction of the several classes there met with, in this respect. The variety there found was most favorable to this end. They were, as was seen, of various types, including the common slide valve with locomotive valve gear; two with automatic valve motions, with balanced valves; one compound engine, condensing and with balanced valve; and the new high speed engine of singular design which has been just described as the "Jarvis engine."

Table IV. gives the log of the straight line engine tested by Professor Carpenter and Mr. Preston, and Table V. that of the 7 × 10 slide valve, the working parts of the standard traction engine of its builders. The trial of the latter was conducted with the "ahead motion" in gear, adjusting the link as required, to secure variation in the point of cut-off, setting it at ¼, ½, and ¾ stroke for the several trials. The driving pulley was 42 in. in diameter, and its weight 320 pounds. The brake had an arm six feet in length. It was found impracticable to prevent some vibration while in operation; but the data may be relied upon as sensibly and satisfactorily correct. Fig. 2 exhibits the method of attachment. The average results were the following:

Friction of 7×10 *Slide Valve Engine.*

Point of Cut-off per cent.	Internal Friction, H. P.
0·25	2·24
0·505	2·75
0·87	2·98

TABLE IV.

STRAIGHT LINE ENGINE, 6′ × 12′. FRICTION WITH CHANGE OF CUT-OFF.

No. of Card.	Boiler Pressure.	Indicator on Crank End. Area.	Length.	M. E. P.	H. P.	Indicator on Head End. Area.	Length.	M. E. P.	H. P.	Total H. P.	Revolutions per Minute.	Rate H. P. to Revolutions.	H. P. Corrected to 295 Revs.	Average H. P.	Cut-off.
1	90	2·8	3·00	2·80	·511	3·0	3·25	2·76	·517	1·08	252	·004	1·08		
2	93	2·8	2·95	2·83	·519	3·0	3·25	2·84	·551	1·07	253	·004	1·12		
3	94	2·2	2·50	2·64	·464	2·9	2·75	2·65	·479	·94	244	·004	1·03	1·08	¼
4	94	2·6	2·70	2·89	·494	2·7	3·15	2·57	·451	·95	237	·004	1·06		
5	93	2·8	3·00	2·80	·448	3·2	3·15	3·10	·509	·96	222	·004	1·13		
6	82	2·6	3·00	2·60	·416	3·0	3·15	2·86	·471	·89	223	·004	1·06		
8	88	4·1	3·15	3·90	·739	4·4	3·00	4·40	·858	1·60	202	·006	1·63		
9	90	3·8	3·15	3·62	·694	4·1	3·00	4·10	·808	1·50	253	·006	1·51	1·42	¾
10	89	3·8	3·15	3·62	·667	3·4	3·00	3·40	·665	1·36	256	·005	1·36		
11	89	3·0	3·15	2·90	·556	3·1	2·90	3 21	·633	1·19	264	·004	1·19		
12	44	3·7	2·70	3·45	·664	3·70	2·70	4·11	·813	1 48	206	·006	1·47		
13	46	3·8	2·85	4·00	·769	4·05	3·20	3·78	·748	1·52	267	·006	1·51		
14	44·5	3·7	2·85	3·89	·748	4·30	3·20	4·05	·795	1·54	267	·006	1·53	1·50	¾
15	44	3·6	2·85	3·81	·731	3·80	3·17	3·57	·660	1·42	295	·005	1·42		
16	42	3·8	2·85	4·00	·761	4·05	3·15	3·97	·757	1·52	282	·006	1·54		
17	39	4·1	2·80	4·38	·818	4·40	3·15	4·17	·800	1·62	264	·006	1·63		
18	39	4·6	2·85	4·83	·901	4·90	3·17	4·65	·893	1·79	259	·007	1·83		
19	40	3·5	2·85	3·69	·705	3·85	3·17	3·63	·713	1·43	295	·005	1·42	1·58	¾
20	41	4·4	2·85	4·62	·862	4·00	3·15	3·81	·748	1·63	295	·006	1·63		
21	41	4·5	2·85	3·63	·668	3·75	3·15	3·57	·701	1·39	295	·005	1·39		

* A paper presented at the eighteenth meeting, Scranton, 1888, American Society of Mechanical Engineers. Advance sheets from Volume x., Transactions.

† Vol. ix., Transactions Am. Soc. M. E., Nov., cclxv.

‡ Trans., Vol. viii., No. 88.

§ New York, J. Wiley & Sons, publishers, 1885.

TABLE V.

TEST FOR FRICTION, WITH CHANGE OF LOAD AND OF POINT OF CUT-OFF.

Lansing Iron Works Engine.

7" × 10" Fraction.

No. of Card.	Steam pressure.	Revs. of engine.	M. E. P. Head.	M. E. P. Crank.	I. H. P. Head.	I. H. P. Crank.	Total.	Net load.	Brake H. P.	Friction H. P.	Cut-off.
1	95	210	.....	.....	.....	.....	2·28	0	0·	2·28	⅞
3	105	210	.....	.....	.....	.....	11·54	66	8·27	3·27	"
4	115	210	.....	.....	.....	.....	9·22	52	6·52	2·70	"
5	115	204	.....	.....	.....	.....	9·50	58	6·74	2·76	"
6	110	200	.....	.....	.....	.....	17·40	121	13·80	3·59	"
7	115	202	.....	.....	.....	.....	17·98	121	13·93	4·05	"
	Average.									3·11	
7	100	230	12·2	12·2	2·91	2·69	5·59	24	3·14	2·36	⅛
8	110	232	12·5	13·0	2·56	2·87	5·43	24	3·18	2·25	"
9	110	235	19·6	20·7	4·34	4·49	8·83	46	6·02	2·81	"
10	110	227	18·6	20·1	4·12	4·38	8·50	46	6·01	2·49	"
11	110	218	41·3	44·2	8·75	9·17	17·92	115	14·38	3·54	"
12	115	232	43·2	47·7	8·28	9·17	17·45	122	14·05	3·40	"
13	115	229	44·3	47·1	9·47	9·86	19·33	132	16·55	2·78	"
14	115	200	54·0	59·7	11·55	12·50	24·05	170	21·32	2·63	"
15	115	200	56·1	62·9	10·91	11·18	22·89	167	19·04	3·85	"
	Average.									2·89	
1	55	216	6·2	4·4	1·30	0·90	2·20	0·	0·	2·20	¼
2	55	216	5·6	4·9	1·18	1·01	2·19	0·	0·	2·19	"
	Average.									2·195	
3	70	220	5·0	5·7	1·03	1·19	2·25	0·	0·	2·25	¼
4	65	218	4·5	5·6	0·95	1·16	2·11	0·	0·	2·11	"
	Average.									2·18	
5	75	212	5·0	6·3	1·03	1·27	2·30	0·	0·	2·30	⅜
6	80	212	5·6	5·8	1·15	1·13	2·28	0·	0·	2·28	"
	Average.									2·29	

FRICTION WITH CHANGE OF LOAD.

Lansing Iron Works.

7×10 Slide Valve.

Cut-off ⅛ Stroke.			Cut-off ⅜ Stroke.		
Speed.	Average F. H. P.	No. Trials.	Speed.	Average F. H. P.	No. Trials.
200	3·24	2	200	3·59	1
202	3·40	1	202	4·05	1
218	3.54	1	204	2·76	1
220	2·78	1	210	2·75	3
227	2"49	1			
228	2·81	1			
230	2·26	1			
232	2·25	1			

TABLE VI.

FRICTION WITH CHANGE OF LOAD.

Automatic Engine, 8" × 12".

Lansing Iron Works.

No. of Card.	Head Indicator. M. E. P.	Head Indicator. H. P.	Crank Indicator. M. E. P.	Crank Indicator. H. P.	Total I. H. P.	Revolutions.	Brake H. P.	Brake load.	Friction H. P.	Cut-off per cent. of Stroke. Head.	Cut-off per cent. of Stroke. Crank.
1	5·2	1·74	3·4	1·10	2·84	220	0·	0·	2·84	18	8
2	3·2	1·07	4·4	1·43	2·50	220	0·	0·	2·50	10	11
3	21·2	6·44	23·1	6·84	13·28	200	10·37	4·55	2·91	18	24
4	19·0	6·85	23·8	7·75	14·10	220	11·41	4·55	2·69	18	28
5	25·1	8·39	29·5	9·64	18·04	220	15·42	6·05	2·62	25	33
6	22·2	7·76	28·3	9·30	17·06	220	14·42	5·75	2·64	22	32
7	28·5	9·53	29·5	9·57	19·10	220	16·42	6·55	2·68	26	31
8	22·8	8·50	23·7	8·83	16·80	235	14·36	5·35	2·44	21	28
9	23·85	9·43	28·5	9·88	19·31	200	16·92	5·85	2·39	22	29
10	21·2	7·73	24·1	8·63	16·35	240	14·69	5·55	1·66	29	28

FRICTION WITH CHANGE OF SPEED.

Revolutions.	Average F. H. P.	No. Trials.
200	2·91	1
220	2·63	6
235	2·44	1
200	2·39	1

TABLE VII.

FRICTION WITH CHANGE OF LOAD.

Automatic Engine, 12"×18".

Lansing Iron Works. Steam Pressure, 80 lb.

No. of Card.	Revolutions per Minute.	Head Indicator. M. E. P.	Head Indicator. H. P.	Crank Indicator. M. E. P.	Crank Indicator. H. P.	Total I. H. P.	Brake Load.	Brake H. P.	Friction H. P.
1	190	8·6	8·40	3·0	2·88	11·20	0·	·	11·20
2	190	8·5	8·30	2·8	2·89	11·19	0·	0·	11·19
3	190	8·5	8·30	2·6	2·50	10·80	0·	0·	10·80
4	180	4·4	4·08	4·6	4·19	8·27	0·	0·	8·27
5	180	4·8	4·44	4·75	4·32	8·76	0·	0·	8·76
6	182	7·75	7·25	6·5	5·99	13·24	11·5	2·79	10·45
7	182	6·7	6·27	7·9	7·28	13·55	11·5	2·79	10·76
8	187	8·1	7 79	7·2	6·81	14·60	21·5	5·35	8·25
9	187	9·25	8·89	8·4	7 95	16·84	22·5	5·61	11·23
10	180	9·5	8·89	10·75	9·90	18·89	23·5	7·89	11·00
11	192	14·3	12·14	7·5	7 29	19·43	46·0	11·78	7·65
12	192	15·25	15·06	5·9	5·73	20·78	49·0	12·54	8·24
13	192	14·4	14·21	7·25	7·04	21·25	49·0	12·54	8·71
14	192	16·5	16·28	5·70	5·54	21·82	49·0	12·54	9·28
15	190	16·3	15 92	9·50	9·13	25·05	72·5	18·37	6·68
16	192	15·9	15·69	10·25	9·96	25·65	72·5	18·56	7·09
17	192	17·75	17·57	10·00	9·96	27·53	77·5	19·84	7·69
18	185	22·5	21·40	16·00	14·98	36·38	115·5	28·49	7·89
19	185	22·75	21·68	16 25	15·26	36·94	120·5	29·72	7·22
20	180	26·0	24·06	18·90	17·21	41·27	142·0	34·08	7·19
21	180	24·4	22·57	20 90	19·04	41·61	142·0	34·08	7·53
22	180	25·6	23·69	23·25	21·22	44·91	150·5	36·12	8·79
23	175	30·1	27·07	34·30	30·37	57·44	210·5	49·11	8·33
24	175	30·9	27·80	34·90	30·90	58·70	209·5	48·89	9·81

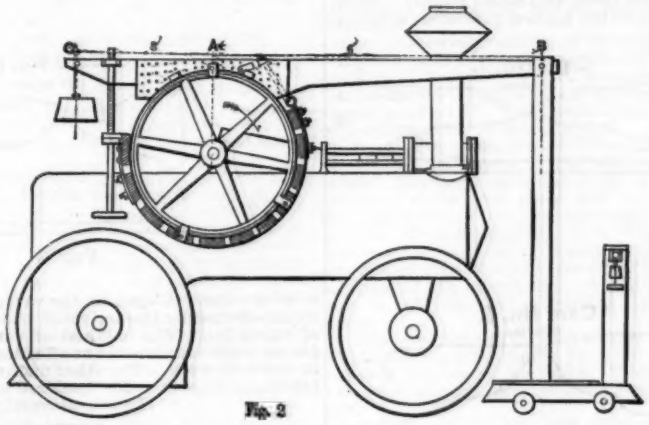

Fig. 2

Plotting the curve thus obtained, it is at once seen that the friction is decidedly increased with increase of load, and decreased in the ratio of expansion. The first seven cards were calculated, assuming the head and back areas of piston equal, and thus obtaining the mean effective H. P.; while the other cards were worked up as indicated in the log. These results with changing cut-off were different from those secured by variation of load at constant ratio of expansion.

Placing a variable load on the brake arm, and thus varying the power from 0 to 21 H. P., the point of cut-off being retained constant, it was found that the friction was very nearly constant, and not variable with the load. The distribution of friction was then ascertained by a careful trial, the results of which are presented in another paper. There, as in the other trials described in these papers, the brake was handled substantially as described in papers already read before the society.

The 8 × 12 automatic engine, which had been in use a year, and was thus well worked into smooth running order, was tested in the erecting room of the shops of the Lansing Co., bolted down on two blocks which were not heavy, but which answered their purpose fairly well. The log of the trial is given in full, and shows the internal friction to have been practically constant for all loads, and to have slightly increased with *decrease* of speed. The speed adjustments were made at the governor.

The 12 × 18 inch automatic engine was a new machine and had not been used; it had only been in operation at the shop long enough to determine the set of its valves and the correctness of its construction. Its internal friction was naturally high, but it is presumed that the variation, if any, would follow the same law as when the engine has attained a smoother condition by long service. The brake arm was 7 feet long, suspended with a counterpoise which removed all its weight from the brake wheel and engine bearings. A continuous stream of water kept its temperature down. The steam supply was not equal to that required by the engine at full power, and it could not, for that reason, be worked at its rated capacity. Its details had the following dimensions: Rod 2¼ in. diameter; main shaft 5 in.; main pulley 6 ft.; weight 1700 pounds; weight of shaft and crank 900 pounds. The log is given below, and the results of the trial indicate the usual condition; nearly constant friction loss, independent, in even its minute variations, of the load on the engine. No sensible change of friction was found, for the changes of speed observed, due to the shortness of steam supply. This engine was also finally taken apart and tested for distribution of internal friction, as elsewhere described, a transmission dynamometer being substituted for the brake used in these trials, the engine driven by an external source of power and part after part taken off to give the friction of each of its important elements.

The tandem compound engine of the same builders was 28 inches stroke of piston, and the two cylinders 14 and 21 inches in diameter, both secured on one rod and driving the same crank. The exhaust steam from the high pressure cylinder is conveyed to the low pressure engine through a pipe, which is also a sleeve for the valve stem of the former. This valve stem passes also through a hollow stem driving the valve of the low pressure engine. The latter is driven by a fixed eccentric, while the high pressure valve is actuated by a loose eccentric, which latter is adjusted momentarily by an automatic system of governing. The regular speed is 200 revolutions per minute, the engine non-condensing. The machine was tested in the erecting shop, like the preceding, and in substantially the same manner. It was new, and its friction waste therefore abnormally high. The same brake was used as before, and it worked admirably and gave, probably, thoroughly reliable data. The friction was 28 H. P., and very constant through the whole series of trials. Three light cards, the engine being entirely unloaded, give an average of 28·85 H. P., and nineteen cards at various loads give an average of 27·33 H. P. The steam pressure was very evenly held at 85 pounds by gauge; but a contracted supply pipe caused some fall of pressure *en route* to the engine at heavy loads. This produced some falling off of speed. The method of operation was, in these tests, usually, to first apply the brake load, then to allow the engine to fully reach the speed desired, and then, when at constant speed, to take indicator diagrams, record of steam pressure, speed, and the brake load as nearly simultaneously as possible. Cards were taken earlier by driving the engine with low pressure cylinder alone, thus securing a measure of its friction as a single cylinder engine; two sets of cards thus taken, light, gave a friction of 32·6 and 34·6 H. P. respectively. Eight cards, the entire engine loaded, gave 20·2 H. P. at 150 revolutions, and 31·6 H. P. at 175 revolutions. The trial on which the conclusions of this paper were based was made later, after several days' operation, and the valves readjusted to give a better distribution of work. The last card obtained was taken two or three weeks later while at regular work. The variation of speed noticed was unaccompanied by any observable variation of internal friction. The following is the log:

TABLE VIII.

FRICTION WITH CHANGE OF LOAD.

Lansing Iron Works. Tandem Compound Engine.

No. of Card.	Revolutions.	High Pressure Cylinder, 14" × 39". M. E. P. Crank.	M. E. P. Head.	I. H. P. Crank.	I. H. P. Head.	Total.	Low Pressure Engine, 21" × 22". M. E. P. Crank.	M. E. P. Head.	I. H. P. Crank.	I. H. P. Head.	Total.	I. H. P. for both Cylinders.	Brake Load.	Brake H. P.	Friction H. P.
2	165	28·3	15·5	29·12	19·88	49·00	1·4	1·8	3·97	2·89	6·86	55·86	123	27·12	28·74
3	168	23·2	15·3	29·52	19·98	49·50	1·3	1·4	3·93	4·12	8·05	57·55	131	29·26	28·29
4	188	27·3	23·7	28·53	25·40	53·93	5·7	5·8	12·06	13·98	26·04	79·97	284	52·15	27·82
5	152	25·7	21·8	29·14	28·10	57·24	5·3	4·9	13·89	13·03	26·92	84·16	290	58·81	25·35
6	168	24·2	21·5	30·80	28·07	58 87	2·8	1·7	9·79	4·99	14 78	73 65	203	45·25	28·40
7	160	25·2	23·8	30·53	29·60	60·13	4·0	3·9	11·04	10·91	21·95	82·08	257	54·88	27 25
8	165	24·6	22·7	30·74	29·11	59·85	3·5	4·1	9·69	11 82	21·51	81 36	252	55·44	25·92
9	165	24·4	22·3	30·47	29·69	60·18	4·0	3·7	11·55	10·63	22·18	82·26	244	53·67	28·69
10	159	25·6	24·3	30·88	30·03	60·86	4·5	4·4	12·34	12·24	24 58	85·44	289	61·27	24·17
11	159	25·5	23·9	30·70	29·55	60·25	4·55	4·8	12·42	13·37	25·79	86·04	292	61·90	24·14
12	183	19·4	14·9	26·38	21·19	47·57	0·55	0·56	1·76	1·78	3·54	51·11	83	20·26	30·85
13	174	22·8	19·4	30·05	26·24	56·29	2·00	1·7	7·00	5·16	12·16	68·45	145	33·75	34·70
14	160	25·9	24·2	31·38	30·10	61·48	4·2	4·9	11·58	13·71	25·29	86·77	263	56·11	30·66
15	162	24·3	23·0	29·82	28·96	58·78	3·7	4·1	10·26	11·61	21·87	80·65	256	55·28	25·37
16	158	25·7	23·4	30·76	28·76	59·53	4·1	4·2	11·18	11·61	22·79	82·31	261	55·00	27·31
17	160	26·5	25·5	32·11	31·73	63·84	4·5	4·5	12·42	12·60	25·02	88·86	298	63·57	25·29
18	156	27·1	25·2	32·00	30·58	62·58	4·6	5·45	12·88	12·56	24·94	87·47	288	58·85	28·62
19	136	27·2	25·0	28·01	26·43	54·44	7·0	6·5	16·46	15·45	31·91	86·35	333	60·38	25·97
20	130	27·4	24·6	26·96	24·88	51·84	7·0	6·8	15·73	15·46	31·19	83·03	353	61·19	21·84
21	156	16·8	4·7	19·88	5·70	25·58	1·0	0·4	2·60	1·09	3·69	29·22	0·	0·	29·22
22	150	12·3	6·8	13·96	7·92	21·88	....	....	....	....	...	...	....	....	....
23	156	14·4	6·6	17·04	8·00	25·04	0·55	0·4	1·49	1·10	2·59	27·63	0·	0·	27·63
24	158	14·9	4·7	17·80	5·76	23·56	1·5	0·75	4·07	2·06	6·13	29·69	0·	0·	29·69
A	190	26·3	26·0	37·92	38·39	76·31	7·9	7·5	25·88	24·91	50·79	127·10	....	....	....

Still another most interesting and important investigation was made upon a compound *condensing* engine, built by the same works, as a part of an electric lighting plant. Several such engines have been constructed, each consisting of independent engines in pairs, the small engine exhausting high pressure steam into the other cylinder steam chest, and the exhaust of the larger passing over into a condenser. The main shafts are connected by a coupling in such manner that they may be at any moment separated and worked independently, should accident to either make it necessary, or should but a small amount of power be needed at any time. The two engines are placed about 25 ft. apart, and connected by a 4 in. pipe. During these trials the brake was attached to the small engine flywheel, and the tests were conducted as nearly as possible in the way already described, the same brake being used as when making trial of the automatic engine last referred to. The high pressure engine, in this case, had an automatic governor, and the low pressure cylinder had an eccentric fixed to cut off at ⅞ stroke. The two engines were first operated independently, and then together. The engine friction was unquestionably independent of the amount of power produced by either or by both engines. The log of these last trials is given below.

TABLE IX.

FRICTION WITH CHANGE OF LOAD.

Lansing Iron Works. Compound Condensing Engine.

No. of Card.	Revolutions.	Low Pressure Cylinder, 22″ × 18″.					High Pressure Cylinder, 21″ × 20″.					I. H. P. for both Cylinders.	Brake Load.	Brake H. P.	Friction H. P.
		M. E. P.		I. H. P.		I. H. P.	M. E. P.		I. H. P.		I. H. P.				
		Head.	Crank.	Head.	Crank.	Total.	Head.	Crank.	Head.	Crank.	Total.				
1	187	....	....	....	....	....	....	....	. ..	....	....	....	....	....	....
2	190	1·00	0·85	3·42	2·61	6·03	....	....	....	....	....	....	0·	0·	....
3	190	1·00	1·15	3·42	3·62	7·04	3·0	4·5	3·50	5·03	8·53	15·57	0·	0·	15·92
4	196	0·05	0·3	1·72	1·03	2·75	6·8	9·5	7·63	10·24	17·87	15·12	0·	0·	15·07
5	196	0·05	0·3	1·72	1·03	2·75	6·1	10·4	6·90	11·25	18·15	15·40	0·	0·	15·35
6	196	0·05	0·3	1·72	1·03	2·75	7·2	8·9	8·10	9·63	17·73	14·97	0·	0·	14·92
7	208	3·05	3·5	12·72	12·48	25·20	29·1	36·0	34·60	41·25	75·85	101·05	310	85·99	15·06
8	208	4·15	4·65	15·10	16·78	31·88	35·5	39·7	42·18	43·75	85·93	117·81	370	102·61	15·20
9	208	2·09	3·0	10·54	10·69	21·23	26·3	31·2	31·27	35·77	67·04	88·27	270	74·74	13·53
10	209	4·00	4·2	14·55	14·97	29·52	34·1	38·0	40·50	44·28	84·78	114·30	360	99·84	15·46
11	208	2·00	2·1	7·28	7·49	14·77	20·0	20·3	23·02	22·55	45·57	60·34	155	41·92	17·42
12	208	2·01	2·2	7·63	7·84	15·47	22·5	28·9	26·77	33·00	59·77	75 24	210	58·24	17·00
13	208	2·03	2·6	8·46	9·34	17·82	28·4	37·5	32·68	42·93	75·61	93·43	280	77·62	15·81
14	208	1·00	1·5	3·64	5·29	8·93	11·3	12·3	11·78	14·10	25·88	34·81	65	18·03	16·78
15	208	0·95	1·3	3·52	4·61	8·13	11·3	12·3	11·78	14·10	25·88	34·01	62	17·20	16·81

The new engine of Mr. Jarvis, to which reference has already been made, afforded an excellent opportunity to secure data of value relating to the method of variation of internal friction of engine, or its constancy. Cards were taken from this engine, up to speed of above 900 revolutions per minute, with the Crosby indicator. Motion was given the instrument through a rod attached to the piston and playing through a stuffing box in the head, and connected to a lever reducing gear. No serious difficulty was experienced in taking diagrams at the highest speeds reached. Some difficulty was experienced in securing efficient lubrication of the brake when the speed exceeded about 600. The friction of the engine, as already remarked, was small, the number of parts being less than usual, and their weight very little. Table X. exhibits the data collected in the second trial —the first has already been given—and Table XI. those obtained in the course of the third test. They are satisfactory as showing the independence of the internal friction of this engine and the load. The effect of speed on changes of friction of engine has already been fully shown. Figs. 4 and 5 show the form of cards taken, and Fig. 3 shows the peculiar design of steam cylinder. Tables XII. *et seq.* are added in order that the results of earlier trials of other engines, and by other observers, may be compared with those derived by this peculiarly valuable series of investigations, which has so fully corroborated the earlier deductions of the writer. In conclusion, the writer would take advantage of this opportunity to express his gratification with the completeness and accuracy of the work of the observers, to record and publish which is the object of this paper, and to acknowledge with great pleasure and satisfaction both the skill and patience of the investigators and the value of their work. The interest of the proprietors and manager of the Lansing Engine Works leading them to take a vast amount of trouble and some expense in aiding the observers, entitles them also to both great credit and hearty thanks.

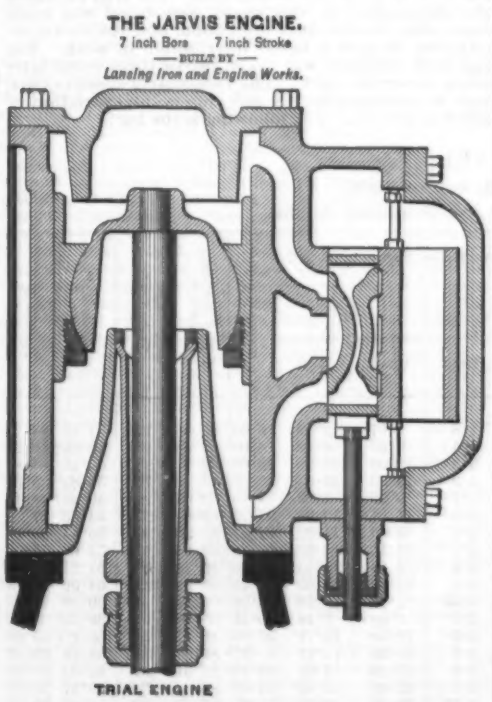

FIG. 3.

Figs. 6 to 9, inclusive, exhibit, better than can any written description, the facts and the phenomena revealed by the several series of investigations which the writer has now had the privilege of presenting, and are a fitting supplement to the text in which the results of such important researches are first published.

Fig. 6 is the graphical representation of Day & Riley's observations, as exhibited in earlier papers of this series, and shows the method of variation of engine friction with change of steam pressure from 20 to 70 pounds per square inch. It is obtained by collating the several series of trials indicated by the different forms of dot on the plate, and shows plainly a variation from the lowest to the highest pressures, at which latter point the variation of power observed is due to other causes, and remains constant as a function of the steam pressure. The considerable range of deviation from the curve taken as representative of the mean is due to varying efficiency of lubrication, probably, and well exhibits the importance of maintaining a good supply of lubricating material and a constant flow of oil. These variations are seen to be less at high than at low pressures; the normal working conditions being approached, the magnitude of the engine friction tends to become more perfectly constant as a function of pressure in the steam chest. The variation of friction indicated at the lower pressures is probably here due,

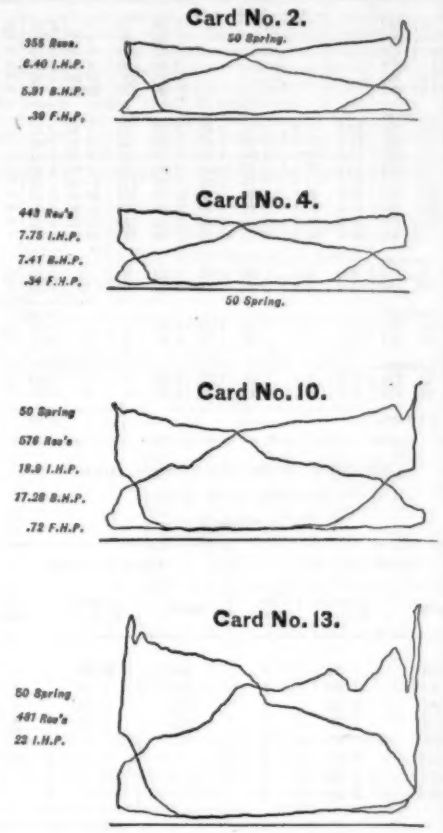

FIG. 5.

to some extent at least, to the varying distribution of steam effected by the action of the automatic system of regulation. This method of variation is obviously not an important matter as affecting ordinary engines in ordinary work. The other methods of variation, as functions of speed and of load, are much more notable.

(To be continued.)

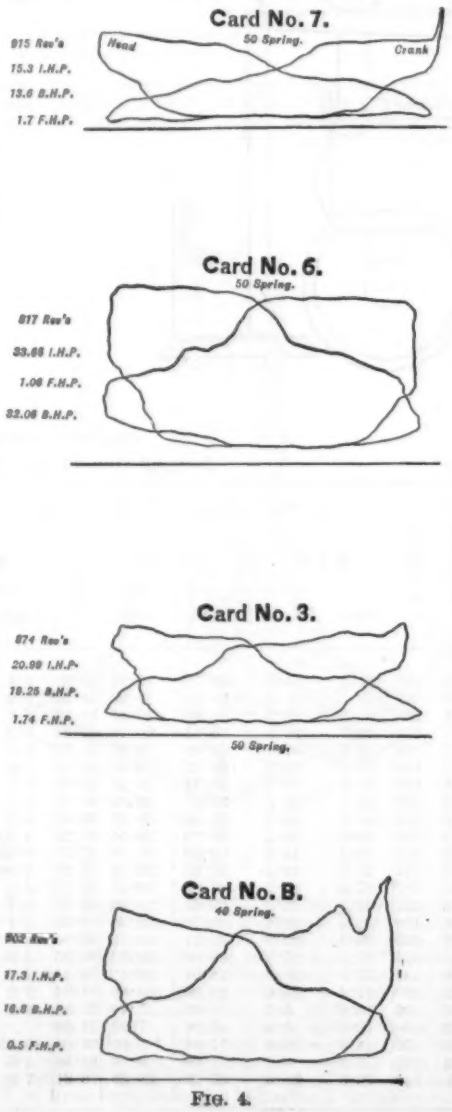

FIG. 4.

THE RELATIVE MERITS OF THE VARIOUS TYPES OF STEAM BOILERS.*

THERE is no branch of the great subject of mechanics that has been more satisfactorily written up and upon which the writers so uniformly agree as that of steam boiler construction and operation. Yet in face of the fact, the principles so carefully demonstrated seem to be but little understood or entirely ignored by those intrusted with the selection and design of steam boiler plants and by steam users generally.

Probably the most common error of the present day is the supposition that excessive heating surface adds to the evaporative capacity of a boiler. The truth is there are well and correctly established proportions for relative heating and grate surface, beyond which there is no advantage to be gained. Equally common and fallacious is the supposition that one particular form or type of boiler will evaporate more water for a pound of coal than a boiler of a different type. Upon this point the authorities all agree that the type of boiler does not necessarily affect evaporative efficiency, and that under like conditions, to which all forms and types may be subjected, like results are attainable.

It should be understood that a steam boiler is merely an instrument designed to receive and transmit heat. Its size is a matter of capacity dependent entirely upon the quantity of heat to be transmitted.

It has been demonstrated that the best results are attained when the heating surface of the boiler is in the proportion of forty square feet to one square foot of grate bar surface, upon which is consumed twelve pounds of coal per hour.

Whether the heating surface of the boiler is the aggregate of tubes, flues, fire box, or shell is quite immaterial, provided the surface of the plates be kept perfectly free from soot, scale and other foreign matter known as non-conductors.

With the consumption of twelve pounds of coal per

*A paper read by Mr. Wm. Golding before the Louisiana Sugar Planters' Association, September 13, 1888.

hour on each square foot of grate there will be produced a certain and constant amount of heat, which will require forty square feet of effective heating surface to absorb, and which, if not absorbed, will pass unutilized to the chimney. It is therefore as essential that the heating surfaces should be kept perfectly clean from soot and scale as that the proportion of forty to one should be provided for in the original design. In regard to form or type of boiler, it has been demonstrated that when the heating surfaces are clean there is really no appreciable difference in evaporative efficiency. Yet it is obvious that while the tubes in the tubular type may be kept comparatively free from soot, they cannot be cleaned on the water side without removal.

It will therefore need no demonstration to show that the tubular type of boiler will from the beginning gradually depreciate in evaporative efficiency. The double flue boiler possesses the merit of being readily cleaned on every part of the inside, and of being kept free from soot on the inside of the flues, which, if carefully done, will preserve the evaporative efficiency of this type of boiler indefinitely.

The plain cylinder boiler, having no flues to keep clean and having the entire interior available for cleaning and for examination and repairs, when correctly proportioned and properly set, possesses all the requirements of a safe and durable boiler of the highest evaporative efficiency.

If it be true, of which there can be no reasonable doubt, that forty to one is the proper relative proportion of heating surface to grate surface, in order to obtain the very best results, and we may here admit that, whether true or not, it will apply as well to one type of boiler as to another.

Let us see how the proportion will work out for the standard plain cylinder boiler, 32 inches in diameter and 32 feet long. Here we have five feet of heating surface to the foot of length, or 160 square feet of heating surface, which, divided by 40, equals 4 square feet of grate surface.

Now, who ever saw such a proportion of grate under a cylinder boiler? and yet it is the correct proportion; and the fact that fully four times as much grate surface would ordinarily be placed under such a boiler does not affect the principle, but only shows the necessity of observing well established laws.

I am aware that in some cases efforts were made to utilize the heat escaping from the excessive grate surface by making the boiler sixty and even eighty feet long, but here again the natural laws relating to combustion and radiation were in conflict, and this line of approach was abandoned.

Let us now examine the proportion of the standard, so called, return tubular boiler, say 63 inches in diameter and 16 feet long, having 63 tubes 16 feet long and 4 inches diameter, and set with 25 square feet of grate surface. Here we have 1,100 square feet of heating surface to 25 square feet of grate surface, or a proportion of 44 to 1. Now, if 44 to 1 will do for tubular, why will it not do for plain cylinder boilers?

The truth is, it will do; and while the results will be precisely the same while both boilers are clean, the tubular boiler will gradually foul and lose its evaporative efficiency, while the plain cylinder boiler, for reasons already given, will maintain indefinitely its original evaporative efficiency.

The foregoing, as will be observed, treats only of steam boiler efficiency, yet, while efficiency is the first quality of a boiler, this quality must yield in certain cases to the quality of utility; for it cannot be expected that any particular type will be the best in every case.

Take for example the steam fire engine, the essential qualities of which are portableness and rapidity in getting up steam. The first quality is accomplished by concentrating by the tubular system the requisite heating surface into a small space, with the smallest amount of material possible, and the latter quality is accomplished by allowing as little space as possible for water. If a boiler is required for unsteady work, such as running a printing press, where the work is but an hour or two in the day, a concentrated system of heating surface will be the most economical, for the reason that there will be but little heat to radiate during the hours of non-use. When the boiler is required to furnish steam for ten or twelve hours a day, as in a factory, the best results will be attained with the double flue boiler, as it possesses every essential quality. If the boiler is required to furnish steam continuously day and night, where excessive weight and space occupied is not objectionable, the plain cylinder type will yield incomparably the best results.

The following will serve to explain the technical terms in the sense in which they are used:

Boiler efficiency, pounds of water evaporated for pounds of coal.

Boiler capacity, pounds of water evaporated per hour.

Boiler power, 30 pounds of water evaporated per hour is one horse power.

Heating surface, that portion of the boiler or flue exposed to the fire or product of combustion.

The heating surface in any type of boiler should be forty square feet to each square foot of grate surface.

The draught at all times should be ample to insure the combustion of twelve pounds of coal per hour per square foot of grate surface.

From a careful perusal of the foregoing, which will be found to accord with the authorities, a careful business man will be able to decide for himself the type and capacity of the boiler best suited to his particular case.

In the early days of the steam boiler its application and use was not restricted by reason of space or weight; efficiency and safety being then the only considerations.

With the advent of the locomotive and the steamship new conditions were required, to meet which the tubular system came into existence, not as an improvement or discovery, but as an expedient, thus giving rise to the error which has become universal, that "the tubular boiler makes the most steam."

We have here a lesson "not dreamed of in philosophy," that while truth, the physical associate of matter, is arrested by the same impeding causes, its opponent, error, seemingly forgotten and unclassed in physics, rolls on with undiminished speed through infinite space.

THE GREAT LAKES.

WE have received from the author, Mr. Charles Crosman, a copy of his valuable chart, 25 × 37 inches, showing graphically the fluctuations of the water surface and the rainfall from 1859 to 1888, line of deepest water, greatest depth, areas of water surfaces and basins, discharge and tides of the great lakes, compiled from official data obtained from the United States Lake Survey, Smithsonian Institution, and War Department. From the accompanying notes given by Mr. Crosman we take the following:

An accurate knowledge of the rise and fall of the surface of the lakes from other causes than by winds is of importance to all interested in their navigation and to owners and occupants of property adjacent thereto.

The rapid development of the grain-growing district west of the head of Lake Superior, and the iron mines on the north and south shores of the lake, having occurred during a period of high water, has stimulated the building of vessels of deeper draught and larger carrying capacity than prevailed a few years ago.

A careful examination of the chart will show that from 1882 to 1888 the surface of Lakes Michigan, Huron, and Erie was considerably above the mean level.

The water in the spring of 1888 was about one foot lower than the average of the period from 1882 to 1887, and, judging the future by the past, it is probable that for several years next ensuing there will be no permanent increase in depth.

During periods of high water, parties interested in navigation are happy, while owners of property on the lake shores are less jubilant as they witness the rapid erosion of the shore and disappearance of their real estate, never more to return. Occupants of stores and warehouses are also subjected to much anxiety and not unfrequently to heavy losses by the flooding of basements and cellars. These losses may be materially lessened by taking the necessary precautionary measures.

The widespread anxiety that the fall of about one foot in the level of the lakes causes to those interested in navigation, and the readiness to attribute it to some mysterious agency, is shown by the following article, taken from the *Marine Record* of April 5, 1888:

LOW WATER.

"The water level in the lakes continues to fall, and people concerned in navigation are beginning to feel serious alarm. The *Sarnia Canadian* of this week says the water fell six inches in the St. Clair River last week, and the fall was common to the whole lake system. At Sault Ste. Marie the water is very low, and boats drawing over 13½ feet will not be able to get through when navigation opens, which means that boats will not be able to carry anywhere near their maximum capacity next season, and that all classes of freights will be higher than usual. The cause of this remarkable state of affairs is a mystery. Whether some immense subterranean outlet has suddenly been afforded for the vast body of water, or whether the tributary streams have been affected by drought, is a matter upon which scientific men are not agreed. The opening of navigation will be looked forward to with interest."

The interesting reports of General O. M. Poe, Lieutenant-Colonel of Engineers, United States Army, under date of February 7, 1888, Assistant Engineer Alfred Noble, March 1, 1882, and an interesting paper by Assistant Engineer L. Y. Schermerhorn, give much valuable information on this subject.

General Poe states that: "There is no indication anywhere that the waters in the lakes have mysteriously fallen. The long-continued series of observations now available show that since 1838 the water level has fluctuated within limits somewhat less than six feet, and that these fluctuations were due to the greater or less rain and snow fall. It may be considered as a fact established that the lakes are simply great pools forming part of the course of a river, and that they conform to all the laws governing the rise and fall of rivers.

"The observations alluded to also show that there is practically no difference of level between the inlet and outlet of any one of the lakes. That is to say, that in a state of rest the surface of the water in each lake is level.

"But the surface is greatly affected by winds, variations in barometric pressure, and other extraneous phenomena."

And Mr. Noble reports as follows:

"I have the honor to transmit herewith a sketch* showing the relative stage of water in Lake Superior and rainfall at Sault Ste. Marie, from November, 1873, to November, 1881.

"The variation in the stage of water in the great lakes has been the subject of much conjecture, some of it intelligent and some absurd. During seasons of high water, people perceive a gradual and permanent rise of water and support the idea by abundant references to the oldest inhabitant. During seasons of low water, a gradual and permanent fall of water is easily perceived, and by the same references as easily proved.

"It is well understood that the principal factors on which the stage of water in the great lakes depends are the rainfall, the evaporation, and winter temperature. The rainfall furnishes the supply, the evaporation withdraws a part before it reaches the mouths of the streams and from the surface of the lakes. A severely and continued cold winter, preventing the melting of snow, will raise the summer stage at the expense of the winter stage.

"The region of the great lakes is so vast that the conditions of rainfall, evaporation, and temperature may vary widely in different sections. Lake Superior, draining a comparatively limited area, receiving water from no other large basin, and having a severe winter climate, will afford the best opportunity for studying the effects of the several factors named when sufficient data have been accumulated.

"At Fort Brady, Sault Ste. Marie, a record of the rainfall has been kept since July, 1873. The record of the stage of water at the foot of Lake Superior has been kept at the St. Mary's Falls Canal since an earlier period. No continuous observations of evaporation have been made in the Lake Superior basin that I am aware of.

* Omitted. Printed in Report of Chief of Engineers, United States Army, for 1882.

"The curve of rainfall is plotted from the tri-monthly means. The mean of rainfall for July, August, and September is plotted in the vertical for September, etc.

Year.	January.	February.	March.	April.	May.	June.
1873	.....	.....	.....	.....	.....	.....
1874	115·60	115·44	115·34	115·21	115·59	116·06
1875	115·83	115·58	115·54	115·45	116·09	116·39
1876	115·77	115·72	115·47	115·50	116·23	116·81
1877	115·97	115·99	115·88	115·75	115·68	115·98
1878	115·60	115·34	115·24	115·23	115·50	115·76
1879	114·95	114·55	114·31	114·44	114·81	114·96
1880	114·46	114·42	114·33	114·35	115·09	115·84
1881	115·33	115·23	115·26	115·14	115·57	115·80
Mns.	115·43	115·28	115·17	115·13	115·57	115·95

Year.	July.	August.	September	October.	November	December
1873	.....	.....	.....	.....	116·39	116·22
1874	116·45	116·49	116·48	116·61	116·39	116·28
1875	116·47	116·50	116·77	116·64	116·36	115·80
1876	117·27	117·32	117·36	117·02	116·74	116·26
1877	116·31	116·42	116·18	116·22	115·98	115·82
1878	115.84	115·83	115·55	115·66	115·57	115·35
1879	115·26	115·28	115·20	115·23	115·05	114·65
1880	116·08	116·00	116·12	115·93	115·91	115·63
1881	116·00	115·91	116·10	116·51	.....	.....
Mns.	116·21	116·22	116·23	116·23	116·05	115·76

"In platting these means the lower line of means is assumed as the base line from which the ordinates are set off. For instance, in January, 1874, the water was 0·17 above the mean of the January means, and the stage of water for that month is represented on the diagram by laying off 0·17 above the base line. The rainfall curve is platted in the same way.

"The relation between the rainfall and the stage of the lake can be perceived unmistakably in the spring, autumn, and summer of 1876, the remarkable rise of water, culminating in September, 1876, corresponding with a period of heavy rainfall. This period was followed by a few months of light rainfall, during which the water fell rapidly. From this time until December, 1879, the rainfall was, as a general thing, less than the mean, and the water's surface had a downward tendency.

"Occasional periods of two or three months of rainfall considerably above or below the mean affected the stage of water in nearly every case.

"In January, 1880, began a period of heavy rainfall and a rise in the water. From June to August, 1881, the rainfall was light and the stage of water a falling one. In September there was the heaviest rainfall known for many years, accompanied by a correspondingly rapid rise in the water.

"The accompanying sketch shows also the curve of temperature platted in the same way from the monthly means. While there is every reason to believe that a winter of continually freezing weather, by retaining the snowfall until the thawing weather of April or May, will tend to raise the summer level of the lake at the expense of the winter level, it is not confirmed to any great degree by this curve. The explanation of this is not difficult. A single week of warm weather in the winter, causing the melting of the greater part of the snow, might be preceded and followed by extremely cold weather, giving a low mean temperature for the month; so that a cold winter does not necessarily imply the impounding until spring, in the form of snow, of the winter rainfall."

Mr. L. Y. Schermerhorn's paper is as follows:

"In the following memoranda an attempt has been made to assemble a part of the latest and most reliable information relating to the great lakes. The lately completed lake surveys, made by the United States, have reduced to exactness much that previously was only approximate, and the perfection of methods used gives a perfection to the results which seldom obtains in surveys covering so great geographical extent and involving so many details.

"The water surface of the great lakes with the land draining into it presents a total drainage basin of over 270,000 square miles, assembled as follows:

	Area of Water Surface, Square Miles.	Area of Water Shed, Square Miles.	Aggregate Area of Basin, Square Miles.
Lake Superior....	31,200	51,600	82,800
St. Mary's River..	150	800	950
Lake Michigan....	22,450	37,700	60,150
Lake Huron and Georgian Bay..	23,800	31,700	55,500
St. Clair River....	25	3,800	3,825
Lake St. Clair....	410	3,400	3,810
Detroit River.....	25	1,300	1,325
Lake Erie	9,960	22,700	32,660
Niagara River....	15	300	315
Lake Ontario.....	7,240	21,600	28,840
	95,275	174,800	270,075

"The water surface of Lake Superior nearly equals the combined areas of New Hampshire, Vermont, Massachusetts, and Connecticut; Lakes Michigan and Huron together nearly equal the area of the State of New York; Lake Erie, the combined areas of New Jersey and Delaware; and Lake Ontario, about three-fourths of the area of Maryland. The combined area of the lakes exceeds the area of England, Wales, and Scotland.

"The drainage basin of the great lakes shown on *

* Figs. 1, 2, 3, and 4 omitted. Printed in American Journal of Science, April, 1887.

Fig. 1 is a reduction of a very careful compilation of the best authorities, and shows at a glance the details stated in the previous table. It shows also that while the line of deepest water does not coincide with the middle line of the lakes, it does approximately coincide with the medial line of lake basin. It will be further observed that, with the exception of Lake Erie, the points of greatest depth quite nearly coincide with the center of figure of each basin. This is believed to be a fact never before noticed, and one that may cast some light upon the geological evolution of the great lakes.

"The length of shore line of the lakes and their connecting rivers is about 5,400 miles, or about equal to the coast line from Maine to the isthmus of Panama, if the distance be measured along the general contour of the coast, ignoring minor indentations.

"The recently perfected levels of the United States lake surveys, between tide water and the lakes, fixes the elevation of their mean surfaces above mean sea level as follows:

Lake Ontario	246 1/10	feet.
" Erie	572 1/10	"
" Huron and Michigan	581 1/10	"
" Superior	601 1/10	"

"The difference of 20 1/6 feet between Lakes Superior and Huron occurs in the rapids of St. Mary's River; the 8 1/10 feet between Lakes Huron and Erie mainly in Detroit River.

"The difference of 326 feet between Lakes Erie and Ontario occurs in the vicinity of Niagara Falls, and is principally assembled as follows (see Fig. 3): 100 feet in the 5 miles of rapids between Lewiston and the lower suspension bridge, 10 feet in the rapids between the bridge and the falls, 160 feet at the falls,* 50 feet in the rapids immediately above the falls, and 6 feet in the upper Niagara River.

"The mean depth of Lake Superior is about 475 feet; the deepest point (see Fig. 2) marks a depth of 1,008 feet, or 406 feet below the level of the sea. Lake Huron has a mean depth of about 250 and a maximum depth of 750 feet. Lake Michigan has a mean depth of 325 feet and a maximum depth of 870 feet. Lake Erie is comparatively shallow, having an average depth of less than 70 feet and a maximum of 210 feet. Lake Ontario has a mean depth of about 300 feet and a maximum of 738, or nearly 500 feet below the level of the sea. The channels of the rivers connecting the lakes seldom exceed the depth of 50 feet. If the lakes could be drained to the level of the sea, Lake Erie would disappear, Lake Huron reduced to quite insignificant dimensions, Lake Michigan to a length of about 100 miles, with a width of 25 or 30 miles, Lakes Ontario and Superior, although with diminished areas, would still preserve the dignity of their present titles as *Great Lakes*.

"A chemical analysis of water taken from the deepest part of Lake Superior failed under the application of delicate tests to indicate the presence of salt.

"The beds of the lakes away from the vicinity of the shore lines, and at depths exceeding 100 feet, are almost invariably covered with clay. Specimens from the deep soundings of Lake Superior were invariably soft clay, varying in color from red to yellow and blue. In the deepest parts the drabs and bluish tints predominate.

"The temperature at the deepest points varies little from the mean annual temperature of the surrounding air. The temperature of Lake Superior at depths exceeding 200 feet varies but slightly from 39° F. In Lake Huron, at depths of about 300 feet, the temperature in the months of June and August was 52° F., while at a depth of 624 feet the temperature was 42° F., the surface temperature being 52° F. and the air 64° F.

"The mean annual rain and melted snow fall of the several lake basins is as follows: Lake Superior, 29 inches; Lake Huron, 30 inches; Lake Michigan, 32 inches; Lakes Erie and Ontario, 34 inches. This is about equal to 31 inches on the entire lake basin.

"The following represents the average discharges at the outlets of the lakes:

	Cubic Feet per Sec.
Lake Superior, at St. Mary's River	86,000
Lakes Michigan and Huron, at St. Clair River	225,000
Lake Erie, at Niagara	265,000
Lake Ontario, at St. Lawrence River	300,000

"The aggregate discharge of the lakes is double that of the Ohio and nearly equals half the discharge of the Mississippi. The area of the lake basin is a third larger than the basin of the Ohio, or about a fifth the combined areas of the basins of the Mississippi and its affluents. The outflow of the lake basin is slightly less than half the rainfall, while on the Mississippi and Ohio the discharge is about a fourth the rainfall. If the average discharge of the lakes passed through a river one mile wide with a mean velocity of one mile per hour, such a river would have a depth of 40 feet from shore to shore.

"The volume of water in the lakes is about 6,000 cubic miles, of which Lake Superior contains a little less than one-half. Perhaps a better idea of this volume may be obtained when it is said that it would sustain Niagara Falls in its present condition for about 100 *years*.

"In relation to the depth of water on the crest of Niagara Falls, it can be easily demonstrated that if the water passed over the falls in a sheet of uniform thickness for the entire length of the present waterway, which is about 3,600 linear feet, the depth of the sheet would not exceed 4 feet. It is highly probable that at the apex of the Horseshoe Falls the depth is nearly 20 feet; consequently, it may be inferred that the depth of the sheet, except in a few places, is less than 4 feet.

"The principal changes in the elevation of the lake surfaces are those due to the wind and to rainfall. Prof. Whittlesey states that on August 18, 1848, a gale from the northeast reduced the water level at Buffalo, N. Y., to a point 15 1/2 feet lower than the surface of the lake on October 18, 1849, at which time a terrible gale occurred from the southwest. This was an excessive difference of level and one of rare occurrence.

"During protracted autumn gales, waves have been observed which, through reliable means, measured from 15 to 18 feet above the normal surface.

"The second class of variations, those due to rainfall, occur with considerable regularity. The lowest water in the year generally occurs in Lakes Ontario, Erie, Michigan, and Huron in the months of November and March, and on Superior in March. The highest water occurs on the first named lakes in June and July, while on Superior it is delayed until September. The average difference, derived from twenty years' observation, between the high and low water of the year is as follows:

"Lake Superior, 1·3 feet; Michigan and Huron, 1·3; Erie, 1·6; and Ontario, 1·9 feet.

"The highest observed stage of the lakes occurred in the summer of 1838, and the lowest in the summer of 1847, the difference between the two stages being about 4 1/2 feet.

"The water level of the lakes for the last twenty years is shown in Fig. 4, and fails to show, as has been claimed, any recurring cycle of high or low water.

"The curves of the last ten years show a tendency to irregularities, which may be due to changes in rainfall and watershed produced by the rapid destruction of the forests which ten years ago covered the basin of the upper lakes.

"Observations made by the United States Survey have established the existence of small tides, which at Chicago had an amplitude of 1 1/2 inches for the neap tide and about 3 inches for the spring tide.

"There is still another class of oscillations called *seiches*, which have been already observed in the Swiss lakes, and for which a solution in all respects satisfactory has not been offered. Whenever the lakes are sufficiently free from the disturbing action of wind to permit observations, a quite regular series of small waves, or pulsations, can be detected, which have an interval of about ten minutes from impulse to impulse. These pulsations seem to occur almost without cessation on Lake Superior. Besides having tides in common with the ocean, the lakes have well defined land and lake breezes; the breeze from the lakes landward commencing in summer at 8 or 10 o'clock A.M. and continuing until sunset, and the breeze from the land lakeward from 9 or 10 P. M. until sunrise.

"The modifying effect of these large bodies of water upon land areas contiguous to the lakes is noticeable. At Milwaukee, Wis., directly upon the shore of Lake Michigan, the mean annual temperature is as follows: Winter, 24° F.; spring, 41° F.; summer, 67° F.; autumn, 49° F.; annual range, 110° F. At points in the same latitude, but from 50 to 100 miles inland, the mean winter and autumn temperatures are about 2° F. lower, and mean spring and summer temperatures nearly 5° F. higher, while the annual range is about 5° F. greater."

The rainfall at a point on the shore of each lake is shown on the chart. It is possible that if observations were taken at many stations, and the means of areas not exceeding 400 or 500 square miles were obtained, the rainfall curve would more nearly coincide with the water level curve, as the amount of rainfall differs greatly at stations but a few miles apart. For example, the precipitation at Chicago in 1885 was 44·37 inches, and at Milwaukee for the same period 32·58 inches, showing a difference of 11·79 inches in a distance of but 85 miles.

EROSION OF THE LAKE SHORES.

Covering a stretch of coast of about 150 miles on the west shore of Lake Michigan, extending from Evanston, Ill., to Manitowoc, Wis., Prof. Edmund Andrews, President of the Chicago Academy of Sciences, in a very interesting paper on the North American lakes, published in 1870 * gave "the average annual erosion for the whole distances at 5·28 feet; from Milwaukee to Manitowoc, average, 4·33 feet a year; from Milwaukee to Evanston, average, 6·24 feet a year; and at various intermediate points as follows:

Evanston	16·95	feet a year.
Winnetke	4·05	" "
Lake Forest	1·65	" "
Waukegan	0·00	" "
State Line	16·50	" "
Kenosha	12·00	" "
Racine Point	16·00	" "
Racine	6·00	" "
Oak Creek	2·00	" "
Milwaukee	6·25	" "
Port Washington	2·30	" "
Sheboygan	6·25	" "
Manitowoc	5·00	" "

"And at Cleveland, O., Lake Erie surveys show that the erosion for forty years has averaged about 6 feet per annum."

To which I add the following of a more recent date:

Mr. G. Y. Wisner, superintendent of lighthouse construction, Detroit, Mich., states that "the data on file in the lighthouse department show that during the period from 1857 to 1878 (twenty-one years) an erosion of about 100 feet took place along the lake front of the L. H. Reservation at North Point, Milwaukee, Wis."

The rapid washing away of the shore at this point caused the abandonment of the lighthouse and the erection of a new structure in 1887.

Under date of April 16, 1888, Mr. Lafayette Towsley, of Port Washington, Wis., states that "the erosion of the lake shore by wave action from 1835 to date is from 25 to 33 feet at Mequon, 20 to 30 feet at Grafton, 28 feet at Port Washington, north of harbor, and 33 feet south and along front of harbor."

Under date of March 26, 1888, Dr. J. J. Brown, of Sheboygan, Wis., writes as follows:

"The annual wearing away of the lake shore here at Sheboygan and vicinity is not far from eight feet, excepting, of course, the immediate vicinity of the north pier of the harbor, and the lighthouse point, which is limestone up to the surface of the water.

"My residence and lot is on the bluff north of the harbor about 2,000 feet; have lived here thirteen years, since which the bank has receeded about 75 feet. North of my home and this side of the lighthouse the high bluff has gone off some faster. South of the harbor for a long way the bank, which is the highest in this region, has gone off still more. During the past forty years we have built three roads through to Black River which the lake has washed away.

"With the exception of the loss of land, no other damage of note has been done."

Under date of April 5, 1888, Mr. John Nagle, of Manitowoc, Wis., states the results of his observations and investigations as follows:

"From what is known as the north harbor pier in the city of Manitowoc to the Little Manitowoc River, a distance of about one-half mile, the lake has encroached upon the land an average of 300 feet within the last thirty years.

"Within the same period the encroachment of the lake on the shore line between Forget-Me-Not Creek and the residence of Mr. E. R. Smith, a distance of one-quarter of a mile, about midway between Two Rivers and Manitowoc, has been 600 feet. From Manitowoc to Smith's, beyond which the shore line has remained intact, the average erosion has been 250 feet; this covers a distance of 3 1/2 miles."

This erosion caused the destruction of a fine graveled toll road.

Mr. C. G. Boalt, of Ahnapee, Wis., states that "the action of the waters of the lake upon the shore has caused an average receding of the shores in that vicinity of from 50 to 60 feet during the past twenty years (from 1868 to 1888)."

NATURE'S BAROMETERS.

THE nervous system of birds is endowed with such sensitiveness that they have the presentiment of the least atmospheric change a long time in advance. Thus, for example, when, after a fine day, the tempest comes on as quick as a flash and surprises the pilot, the mews and gulls were already expecting it. There they were, upon the reefs, with inquiet air, bustling about, spreading their wings, and uttering plaintive cries.

The barometer has not fallen, all is calm in nature, the sky is clear, there is scarcely a dot on the horizon, and yet the black petrel, the sinister messenger of death, the friend of the tempest, which is seen near ships only when danger is imminent, has come forth from his somber abode, and is skimming the wave with his rapid wing to see whether the billows are bringing him any victims. All at once, the earth trembles, the foliage is agitated by the wind, the billows foam, and the lightning flashes. The bird had foreseen the storm.

At the approach of rain, the martins and swallows fly low, the finch utters a sad and plaintive note; the domestic goose comes and goes, opens its wings, flies, enters the water, and manifests great inquietude. The pea-fowl utters frequent cries, the gum woodpecker moans, the paroquet chatters, the guinea-fowl goes to roost, the frog is silent, the toad hops about, the flowers have a strong and penetrating odor, and many of them close. On the contrary, when it is fine weather, the birds twitter, the redbreast sings at the top of the highest trees, the swallow soars to the clouds, the lark leaves the field and flies into the air singing; the chirp of the cricket is heard, the tree frog climbs the tree, the perfume of the plants is fainter, and the flowers expand.

Upon my window, I have an immortelle held by a pin. This opens in fine weather and closes when it rains.

When the spider ceases to work on its web, it is a sign of rain. If it continues or rebegins its snare during the night, fine weather is near at hand. At the approach of wind or rain, it considerably shortens the threads that suspend its work, and leaves them thus until the return of fine weather. The length of these threads is capable of serving as a sign by which to know the duration of fine or stormy weather. The spider makes changes in its web every day. If it makes these before sunset, the night will be fine and clear. Hence, doubtless the old adage: "Araignée du soir, espoir!" When gossamer floats in the air, it will be fine weather.

If, when the rain is falling, fowls do not seek shelter, but continue to look for food, the rain will last all day. If they take to flight at the first drops, the rain will be of short duration. When but one magpie leaves the nest, it is a sign of rain. When the male and female quit it in company, it is a sign of fair weather.

When a circle surrounds the sun or moon, when the clouds are yellowish in the west, when there are dense, dark fogs, it means rain; while the moon in a clear sky, an evening rainbow, white fogs, and red clouds mean fine weather.

In winter, if the sky is blue and the stars are bright, it is a certain sign of a series of fine days. If the sky becomes limpid or has a whitish appearance, and the stars pale, there will be rain the next day.

Of all known animals, the frog is certainly the most nervous, and the one that has most contributed toward founding the science of galvanism. As I write these lines I have before me a pretty little green one, which charms my moments of leisure by its gymnastic exercises. It is the common tree frog (*Hyla arborea*). It is here upon my desk in a glass jar half full of water . . . and rests upon a partially submerged ladder of ferns. . . The top of the jar is covered with gauze to prevent the escape of the prisoner. By this means I have obtained an infallible barometer.

Every time that I see my frog climb up the ladder, I say to myself, "We are going to have fine weather." When it descends, I say, "Look out for rain." If it remains at the middle of the jar, I do not go out without my umbrella, and, when every one around me is drenched to the skin, I go about in good spirits under my shelter.

The following is what the great book of nature teaches us. If, three or four days after the renewing of the moon, the latter is very clear, it is a sign of fine weather, which will last. If, on the second or third day of the new moon, the horns are blunt, it is a sign of a near rain. If the disk is very red, it is a sign of bad weather. In the first quarter, if it exhibits no black spots, it is a sign of fine weather. When the full moon is clear without black spots and without a red circle around it, it is a sign of fine weather.

If, on the contrary, a few black spots be seen upon its disk and two or three black, thick circles around it, a large amount of rain will fall, and it will be very nasty weather. In summer, when the moon appears red upon rising, it prognosticates very great heat. When it is clear on rising, we have only fine weather to expect.

A sky everywhere serene is a sign of fine weather.—*J. Boisgreppier, in Science en Famille.*

A FINNISH engineer, Herr Ringbom, has built a model submarine boat propelled by electricity, that can be steered accurately in horizontal directions, as well as up and down. It is four feet long and is considered a success. It is to undergo further test at Helsingfors.

* The height of the falls is 155 feet on the Canadian side, 161 feet near the site of Terrapin Tower, and 169 feet near the east side of the American Falls.

* See Transactions of the Chicago Academy of Sciences, Vol. 2,

FILTRATION WITH FILTERS OF METALLIC FELT.

By CHARLES E. MUNROE.

To produce this filter I precipitated platinum chloride with ammonium chloride, using a slight excess of the reagent, and washed the salt thoroughly, first with water and then with alcohol. Then taking a Gooch filtering crucible of platinum, I placed it on a thick piece of bibulous paper and poured in the moist salt. The alcohol ran through the perforations in the crucible and was absorbed by the bibulous paper, while the salt remained behind, the filtering crucible being filled in this way to a height of about 5 mm. The crucible was then carefully cleaned and dried, the cap and cover put on, and the whole ignited, thus driving off the volatile constituents and leaving a spongy, coherent residue of metallic platinum well distributed over the perforated bottom of the crucible.

In order to prevent the sponge from cracking and curling, it was found essential to dry and ignite the salt quite cautiously, but in some instances where cracks did appear they were easily and effectually closed by gently rubbing with a glass rod, while in other cases the crevices were filled with a fresh portion of the ammonium platinic chloride and the drying and igniting repeated. The latter method was also employed when it was thought desirable to increase the thickness of the felt. After a short trial the preparation of the felt became quite easily and readily accomplished. The prepared filters are used in the same way as Gooch's asbestos filters are, the caoutchouc gasket described in my paper on the "Use of Porous Cones in Filtration"* being employed to secure an air tight joint. The platinum felt may be easily removed, when desired, by means of a spatula, the small portions remaining in the perforations being pushed out with the point of a needle.

I have repeatedly prepared these filters in the manner above described, and have filtered with them such preparations as freshly precipitated barium sulphate to my entire satisfaction. Not only that, but by treating the precipitates with their proper solvents I have been able to clean the felt so that the same filter could be used repeatedly. The following data illustrates such use of this filter:

	Grms.
Weight of crucible and felt after first ignition	31·0612
Weight of crucible and felt after second ignition	31·0607
Weight of crucible and felt after rubbing with glass rod to remove thin spots	31·0607
Weight of crucible and felt after third ignition	31·0607
Weight of ditto after 250 cm. of water have been passed through ten times	31·0607
Weight of ditto after freshly precipitated CaC_2O_4 has been filtered upon it and ignited	31·5385
Weight of ditto after treatment with HCl, washing, and igniting	31·0634
Weight of ditto after same operation repeated	31·0609
Weight of ditto after same operation repeated	31·0608
Weight of ditto after $BaSO_4$ has been filtered upon it and ignited	31·4403
Weight of ditto after dissolving the $BaSO_4$ in hot concentrated H_2SO_4	31·0640
Weight of ditto after second treatment with H_2SO_4	31·0609
Weight of crucible complete	30·6744
Weight of felt	0·3865

Felts of this character may of course be obtained by similar means from easily decomposable salts of other metals than platinum, and such felts may be found to have useful applications.

It is probable that this method of producing a platinum sponge within a platinum tube may find useful applications in organic chemistry in addition to its use as a filter.—*Journal of Analytical Chemistry*, vol. ii., Part 3.

THE HISTORY OF A SCIENTIFIC DOCTRINE.†

By S. P. LANGLEY.

"MAN, being the servant and interpreter of nature, can do and understand so much, and so much only, as he has observed, in fact or in thought, of the course of nature. Beyond this he neither knows anything nor can do anything."—BACON'S *Novum Organum*, aphorism 1.

IN these days, when a man can take but a very little portion of knowledge to be his province, it has become customary that your president's address shall deal with some limited topic, with which his own labors have made him familiar, and accordingly I have selected as my theme the history of our present views about radiant energy, not only because of the intrinsic importance of the subject, but because the study of this energy in the form of radiant heat is one to which I have given special attention.

Just as the observing youth, who leaves his own household to look abroad for himself, comes back with the report that the world, after all, is very like his own family, so may the specialist, when he looks out from his own department, be surprised to find that, after all, the history of the narrowest specialty is amazingly like that of scientific doctrine in general, and contains the same lessons for us. To find some of the most useful ones, it is important, however, to look with our own eyes at the very words of the masters themselves, and to take down the dusty copy of Newton or Boyle or Leslie, instead of a modern abstract; for, strange as it may seem, there is something of great moment in the original that has never yet been incorporated into any encyclopedia, something really essential in the words of the man himself which has not been indexed in any text book, and never will be.

It is not for us, then, here to-day, to try

> "How index learning turns no student pale,
> Yet holds the eel of science by the tail,"

but, on the contrary, to remark that from this index learning, from these histories of science and summaries of its progress, we are apt to get wrong ideas of the very conditions on which this progress depends. We often hear it, for instance, likened to the march of an army toward some definite end; but this, it has seemed to me, is not the way science usually does move, but only the way it seems to move in the retrospective view of the compiler, who probably knows almost nothing of the real confusion, diversity, and retrograde motion of the individuals comprising the body, and only shows us such parts of it as he, looking backward from his present standpoint, now sees to have been in the right direction.

I believe this comparison of the progress of science to that of the army which obeys an impulse from one head has more error than truth in it; and though all similes are more or less misleading, I would almost prefer to ask you to think rather of a moving crowd, where the direction of the whole comes somehow from the independent impulses of its individual members, not wholly unlike a pack of hounds, which, in the long run, perhaps catches its game, but where nevertheless, when at fault, each individual goes his own way by scent, not by sight, some running back and some forward; where the louder voiced bring many to follow them, nearly as often in a wrong path as in a right one, where the entire pack even has been known to move off bodily on a false scent; for this, if a less dignified illustration, would be one which had the merit of having a considerable truth in it, but one left out of sight by the writers of books.

At any rate, the actual movement has been tortuous, or often even retrograde, to a degree of which you will get no idea from the account in the text book or encyclopedia, where, in the main, only the resultant of all these vacillating motions is given. With rare exceptions, the backward steps—that is, the errors and mistakes, which count in reality for nearly half, and sometimes for more than half, the whole—are left out of scientific history, and the reader, while he knows that mistakes have been made, has no just idea how intimately error and truth are mingled in a sort of chemical union, even in the work of the great discoverers, and how it is the test of time chiefly which enables us to say which is progress when the man himself could not. If this be a truism, it is one which is often forgotten, and which we shall do well to here keep before us.

This is not the occasion to review the vague speculations of the ancient natural philosophers from Aristotle to Zeno, or to give the opinion of the schoolmen on our subject. We take it up with the immediate predecessors of Newton, among whom we may have been prepared to expect some obscure recognition of heat as a mode of motion, but where it has been, to me at least, surprising, on consulting their original works, to find how general and how clear an anticipation of our modern doctrine may be fairly said to exist. Whether this early recognition of the atomic and vibratory theories be a legacy from the Lucretian philosophy, it is not necessary to here consider. The interesting fact, however it came about, is the extent to which seventeenth century thought is found to be occupied with views which we are apt to think very recent.

Descartes, in 1664, commences his "Le Monde" by a treatise on the propagation of light, and what we should now call radiant heat, by vibrations, and further associates this view of heat as motion with the distinct additional conception that in the cause of light and radiant heat we may expect to find something quite different from the sense of vision or of warmth; and he expresses himself with the aid of the same simile of sound employed by Draper over two hundred years later. The writings of Boyle on the mechanical production of heat contain illustrations (like that of the hammer driving the nail, which grows hot in proportion as its bodily motion is arrested) which show a singularly complete apprehension of views we are apt to think we have made our own; and it seems to me that any one who consults the originals will admit that, though its full consequences have not been wrought out till our own time, yet the fundamental idea of heat as a mode of motion is so far from being a modern one, that it was announced in varying forms by Newton's immediate predecessors, by Descartes, by Bacon, by Hobbes, and in particular by Boyle, while Hooke and Huyghens merely continue their work, as at first does Newton himself.

If, however, Newton found the doctrine of vibrations already, so to speak, "in the air," we must, while recognizing that in the history of thought the new always has its root in the old, and that it is not given even to a Newton to create an absolutely new light, still admit that the full dawn of our subject properly begins with him, and admit, too, that it is a bright one, when we read in the "Optics" such passages as these:

"Do not all fixed bodies, when heated beyond a certain degree, emit light and shine, and is not this emission performed by the vibrating motions of their parts?" And again: "Do not several sorts of rays make vibrations of several bignesses?" And still again: "Is not the heat conveyed by the vibrations of a much subtler medium than air?"

Here is the undulatory theory; here is the connection of the ethereal vibrations with those of the material solid; here is "heat as a mode of motion;" here is the identity of radiant heat and light; here is the idea of wave lengths. What a step forward this first one is! And the second?

The second is, as we know, backward. The second is the rejection of this, and the adoption of the corpuscular hypothesis, with which alone the name of Newton (a father of the undulatory theory) is, in the minds of most, associated to-day.

Do not let us forget, however, that it was on the balancing of arguments from the facts then known that he decided, and that perhaps it was rather an evidence of his superiority to Huyghens that, apprehending before the latter, and equally clearly, the undulatory theory, he recognized also more clearly that this theory as then understood failed utterly to account for several of the most important phenomena.

With an equally judicial mind, Huyghens would perhaps have decided so too, in the face of difficulties, all of which have not been cleared up even to-day.

These two great men, then, each looked around in the then darkness as far as his light carried him. All beyond that was chance to each; and fate willed that Newton, whose light shone farther than his rival's, found it extend just far enough to show the entrance to the wrong way. He reaches the conclusion that we all know; and with the result on other men's thought that, light being conceded to be material, heat, if affiliated to light, must be regarded as material too, for we may see this strange conclusion drawn from experiments of Herschel a century later.

It would seem that the result of this unhappy corpuscular theory was more far-reaching than we commonly suppose, and that it is hardly too much to say that the whole promising movement of that age toward the true doctrine of radiant energy is not only arrested by it, but turned the other way; so that in this respect the philosophy of fifty years later is actually farther from the truth than that of Newton's predecessors.

The immense repute of Newton as a leader, on the whole so rightly earned, here leads astray others than his conscious disciples, and, it seems to me, affects men's opinions on topics which appear at first far removed from those he discussed. The adoption of phlogiston was, as we may reasonably infer, facilitated by it, and remotely Newton is perhaps also responsible in part for the doctrine of caloric a hundred years later. After him, at any rate, there is a great backward movement. We have a distinct retrogression from the ideas of Bacon and Hobbes and Boyle. Night settles in again on our subject almost as thick as in the days of the schoolmen, and there seems to be hardly an important contribution to our knowledge, in the first part of the eighteenth century, due to a physicist.

"Physics, beware of metaphysics," said Newton—words which physicists are apt so exclusively to quote, that it seems only due to candor to observe that the most important step, perhaps, in the fifty years which followed the 'Optics" came from Berkeley, who, reasoning as a metaphysician, gave us during Newton's lifetime a conception wonderfully in advance of his age. Yet the "New Theory of Vision" was generally viewed by contemporary philosophers as only an amusing paradox, while "coxcombs vanquished Berkeley with a grin;" and this contribution to science—an exceptional if not a unique instance of a great physical generalization reached by *a priori* reasoning—though published in 1709, remains in advance of the popular knowledge even in these closing years of the nineteenth century.

In the meantime a new error had risen among men—a new truth, as it seemed to them, and a thing destined to have a strong reflex action on the doctrine of radiant energy. It began with the generalization of a large class of phenomena (which we now associate with the action of oxygen, then of course unknown)—a generalization useful in itself, and accompanied by an explanation which was not in its origin objectionable. Let us consider, in illustration, any familiar instance of oxidation, and try to look first for what was reasonable in the eighteenth-century views of the cause of such phenomena.

A piece of dry wood has in it the power of giving out heat and light when set on fire; but after it is consumed there is left of it only inert ashes, which can give neither. Something, then, has left the wood in the process of becoming ashes; virtue has gone out of it, or, as we should say, its potential energy has gone.

This is so far an important observation, extending over a wide range of phenomena, and, if it had presented itself to the predecessors of Newton, it would probably have been allied to the vibratory theories, and become proportionately fruitful. But to his disciples, and to chemists and others, who, without being perhaps disciples, were like all then, more or less consciously influenced by the materiality of the corpuscular theory, it appeared that this also was a material emanation, that this energy was an actual ingredient of the wood—a crudeness of conception which seems most strange to us, but it is not perhaps unaccountable in view of the then current thought.

I have said that the progress of science is not so much that of an army as of a crowd of searchers, and that a call in a false direction may be responded to, not by one only, but by the whole body. In illustration, observe that during the greater part of the entire eighteenth century this doctrine was adopted by almost every chemist and by most physicists. It had quite as general an acceptance among scientific men then as the kinetic theory of gases, for instance, has now, and so far as time is any test of truth, it was tested more severely than the kinetic theory has yet been; for it was not only the lamp and guide of chemists, and to a great extent of physicists also, but it remained the time-honored and highest generalization of chemico-physical science for over half a century, and it was accepted not so much as a conditional hypothesis as a final guide and a conquest for truth which should endure always. And now where is it? Dissipated so utterly from men's minds that, to the unprofessional part of even an educated audience like this, "phlogiston," once a name to conjure with, has become an unmeaning sound. There is no need to insist on the application of the obvious moral to hypotheses of our own day. I have tried to recall for a moment all that "phlogiston" meant a little more than a hundred years ago, partly because it seems to me that, though a chemical conception, physics is not wholly blameless for it, but chiefly because before it quitted the world it appears to have returned to physics the wrong in a multiplied form by generating an offspring specially inimical to true ideas about radiant heat, and which is represented by a yet familiar term. I mean "caloric."

This word is still used loosely as a synonym for heat, but has quite ceased to be the very definite and technical term it once was. To me it has been new to find that this so familiar word "caloric," so far as my limited search has gone, was apparently coined only toward the last quarter of the last century. It is not to be found in the earliest edition of Johnson's Dictionary, and, as far as I can learn, appears first in the corresponding French form in the works of Fourcroy. It expressed an idea which was the natural sequence of the phlogiston theory, and which is another illustration that the evil which such theories do lives after them.

"Caloric" first seemingly appears, then, as a new word coined by the French chemists, and meant originally to signify the unknown cause of the sensation heat, without any implication as to its nature. But words, we know, though but wise men's counters, are the money of fools; and this one very soon came to commit its users to an idea which was more likely to have had its origin in the mind of a chemist at that time than of any other—the idea of the cause of heat

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* *American Journal of Science*, vol. i. [3], May, 1871.

† Address before the American Association for the Advancement of Science, at Cleveland, O., Aug. 15, 1888, by Professor S. P. Langley, the retiring president of the association.

as a material ingredient of the hot body; something not, it is true, having weight, but which it would have been only a slight extension of the conception to think might one day be isolated by a higher chemical art, and exhibited in a tangible form.

We may desire to recognize the perverted truth which usually underlies error, and gives it currency, and be willing to believe that even "caloric" may have had some justification for its existence; but this error certainly seems to have been almost altogether pernicious for nearly the next eight years, and down even to our own time. With this conception as a guide to the philosophers of the last years of the eighteenth century, it is not, at any rate, suprising if we find that at the end of a hundred years from Newton the crowd seems to be still going constantly farther and farther away from its true goal.

Although Provost gave us his most material contribution about 1790, we have, it seems to me, on the whole little to interest us during that barren time in the history of radiant energy called the eighteenth century—a century whose latter years are given up, till near its very close, to bad *a priori* theories in our subject, except in the work of two Americans; for in the general dearth at this time of experiments in radiant heat, it is a pleasure to fancy Benjamin Franklin sitting down before the fire, with a white stocking on one leg and a black one on the other, to see which leg would burn first, and to recall again how Benjamin Thompson (Count Rumford) not only weighed "caloric" literally in the balance and found it wanting, but made that memorable experiment in the Munich foundries which showed that heat was perpetually and without limit created from motion.

It was in the last years of the century, too, that he provided for the medal called by his name, and which, though to be given for researches in heat and light, has, I believe, been allotted in nearly every instance to men who, like Leslie, Malus, Davy, Brewster, Fresnel, Melloni, Faraday, Arago, Stokes, Maxwell, and Tyndall, have contributed toward the subject of radiant energy in particular.

We observe that till Rumford's time the scientific literature of the century scarcely considers the idea even of radiant heat, still less of radiant energy; so that we have been obliged here to discuss the views of its physicists about heat in general, heat and light in most eighteenth century minds being distinct entities. We must remember, then, to his greater honor, that the idea of radiant heat as a separate study has before Rumford scarcely an existence; all the ways for pilgrims to this special shrine of truth being barred, like those in Bunyan's allegory, by two unfriendly monsters who are called Phlogiston and Caloric, so that there are few scientific pilgrims who do not pay them toll.

The doctrine of caloric is, however, even then recognized as a chemical hypothesis, rather than one acceptable to physicists, some of whom still stand out for vibratory theories even through the darkest years of the century; and, further, we may find, on strict search, that the old idea of heat as a mode of motion has not so utterly died that it does not appear here and there during the last century, not only among philosophers, but even in a popular form.

In an old English translation of Father Regnault's compilation on physics, dated about 1730, I find the most explicit statement of the doctrine of heat as a mode of motion. Here heat is defined (with the aid of a simile due, I believe, to Boyle) as "any agitation whatever of the insensible parts. Thus a nail which is drove into the wood by the stroke of a hammer does not appear to be hot, because its immediate parts have but one common movement. But should the nail cease to drive, it would acquire a sensible heat, because its insensible parts which receive the motion of the hammer now acquire an agitation every way rapid." We certainly must admit that the user of this illustration had just and clear ideas; and the interesting point here appears to be that as Father Regnault's was not an original work, but a mere compendium or popular scientific treatise of the period, we see, if only from this instance, that the doctrine of heat as a mode of motion was not confined to the great men of an earlier or a later time, but formed a part of the common pabulum during the eighteenth century to an extent that has been singularly forgotten.

The last years of the eighteenth century were destined to see the most remarkable experiments in heat made in the whole of the hundred; for the memoir of Rumford appeared in the Philosophical Transactions for 1798; and in the very year 1800 appeared in the same place Sir William Herschel's paper, in which he describes how he placed a thermometer in successive colors of the solar spectrum, finding the heat increase progressively from the violet to the red, and increase yet more beyond the red where there was no color or light whatever; so that there are, he observes, invisible rays as well as visible. More than that, the first outnumber the second; and these dark rays are found in the very source and fount of light itself. These dark rays can also be obtained, he observes, from a candle or a piece of non-luminous hot iron, and, what is very significant, they are found to pass through glass, and to be refracted by it like luminous ones.

And now Herschel, searching for the final verity through a series of excellent experiments, asks a question which shows that he has truth, so to speak, in his hands—he asks himself the great question whether heat and light be occasioned by the same or different rays.

Remember the importance of this (which the querist himself fully recognized); remember that, after long hunting in the blindfold search, he has laid hands, as we now know, on the truth herself, and then see him—let go. He decides that heat and light are not occasioned by the same rays, and we seem to see the fugitive escape from his grasp, not to be again fairly caught till the next generation.

I hardly know more remarkable papers than these of Herschel's in the Philosophical Transactions for 1800, or anything more instructive in little men's success than in this great man's failure, which came in the moment of success. I would strongly recommend the reading of these remarkable original memoirs to any physicist who knows them only at second hand.

One more significant lesson remains, in the effect of this on the minds of his contemporaries. Herschel's observation is to us almost a demonstration of the identity of radiant heat and light; but now, though the nineteenth century is opening, it is with the doctrine still in the minds of most physicists, and perhaps of all chemists, that heat is occasioned by a certain material fluid. Phlogiston is by this time dead or dying, but caloric is very much alive, and never more perniciously active than now, when, for instance, years after Herschel's observation, we find this cited as "demonstrating the existence of caloric," which was, it seems, the way it looked to a contemporary.

In the year 1804 appeared what should be a very notable book in the history of our subject, written by Sir John Leslie, whose name survives perhaps in the minds of many students chiefly in connection with the "cube" which is still called after him.

Leslie, however, ought to be remembered as a man of original genius, worthy to be mentioned with Herschel and Melloni; and his, too, is one of the books which the student may be recommended to read, at least in part, in the original; not so much for the writer's instructive experiments (which will be found in our text books) as for his most instructive mistakes, which the text book will probably not mention.

He began by introducing the use of the simple instrument which bears his name, and a new and more delicate heat measure (the differential thermometer); and with these, and concave reflectors of glass and metal, he commenced experiments in radiant heat, than which, he tells us, no part of physical science then appeared so dark, so dubious, and so neglected. It is interesting, and it marks the degree of neglect he alludes to, that his first discovery was that different substances have different radiating and absorbing powers. It gives us a vivid idea of the density of previous ignorance that it was left to the present century to demonstrate this elementary fact, and that Leslie, in view of such discoveries, says, "I was transported at the prospect of a new world emerging to view."

Next he shows that the radiating and absorbing powers are proportional, next that cold as well as heat seems to be radiated, and next undertakes to see whether this radiant heat has any affinity to light.

He then experiments in the ability of radiant heat to pass through a transparent glass, which transmits light freely, and thinks he finds that none does pass. Radiant heat with him seems to mean heat from non-luminous sources; and the ability or non-ability of this to pass through glass is to Leslie and his successors a most crucial test, and its failure to do so a proof that this heat is not affiliated to light.

Let us pause a moment here to reflect that we are apt to unconsciously assume, while judging from our own present standpoint, where past error is so plain, that the false conclusion can only be chosen by an able, earnest, conscientious seeker, after a sort of struggle. Not so. Such a man is found welcoming the false with rapture as very truth herself.

"What, then," says Leslie, "is this calorific and frigorific fluid after which we are inquiring? It is not light, it has no relation to ether, it bears no analogy to the fluids, real or imaginary, of magnetism and electricity. But why have recourse to invisible agents? *Quod petis, hic est.* It is merely the ambient AIR."

The capitals are Leslie's own, but ere we smile with superior knowledge let us put ourselves in his place and then we may comprehend the exultation with which he announces the identity of radiant heat and common air, for he feels that he is beginning a daring revolt against the orthodox doctrine of caloric; and so he is.

The first five years of this century are notable in the history of radiant energy, not only for the work of Leslie and for the observation by Wollaston, Ritter, and others of the so-called "chemical" rays beyond the violet, but for the appearance of Young's papers, re-establishing the undulatory theory, which he indeed considered in regard to light, but which was obviously destined to affect most powerfully the theory of radiant energy in general.

We are now in the year 1804, or over a century and a quarter since the corpuscular theory was emitted, and during that time it has gradually grown to be an article of faith in a sort of scientific church, where Newton has come to be looked on as an infallible head, and his views as dogmas, about which no doubt is to be tolerated; but if we could go back to Cambridge in the year 1668, when the obscure young student, in no way conscious of his future pontificate, takes his degree (standing twenty-third on the list of graduates), we should probably find that he had already elaborated certain novel ideas about the undulatory theory of light, which he at any rate promulgates a few years later, and afterward, pressed with many difficulties, altered, as we now know, to an emissive one.

Probably, if we could have heard his own statement then, he would have told how sorely tried he was between these two opinions, and, while explaining to us how the wavering balance came to lean as it did, would have admitted, with the modesty proper to such a man, that there was a great deal to be said on either side. We may, at any rate, be sure that it would not be from the lips of Newton himself that we should have had this announced as a belief which was to be part of the rule of faith to any man of science.

But observe how, if science and theology look askance at each other, it is still true that some scientific men and some theologians have, at any rate, more in common than either is ready to admit; for at the beginning of this century Newton's followers, far less tolerant than their master, have made out of this modest man a scientific pontiff, and out of his diffident opinions a positive dogma, till, as years go on, he comes to be cited as so infallible that a questioning of these opinions is an offense deserving excommunication.

This has grown to be the state of things in 1804, when Young, a man possessing something of Newton's own greatness, ventures to put forward some considerations to show that the undulatory theory may be the true one, after all. But the prevalent and orthodox scientific faith was still that of the material nature of light; the undulatory hypothesis was a heresy, and Young a heretic. If his great researches had been reviewed by a physicist or a brother worker, who had himself trodden the difficult path of discovery, he might have been treated at least intelligently; but then, as always, the camp followers, who had never been at the front, shouted from a safe position in the rear to the man in the dust of the fight that he was not proceeding according to the approved rules of tactics; then, as always, these men stood between the public and the investigator, and distributed praise or blame.

If you wish to hear how the scientific heretic should be rebuked for his folly, listen to one who never made an observation, but, having a smattering of everything books could teach about every branch of knowledge, was judged by himself and by the public to be the fittest interpreter to it of the physical science of his day. I mean Henry Brougham, the future lord-chancellor of England, the universal critic, of whom it was observed that "if he had but known a little law, he would have known a little of everything." He uses the then all-powerful *Edinburgh Review* for his pulpit, and notices Young's great memoir as follows:

"This paper contains nothing which deserves the name either of experiment or discovery; and it is, in fact, destitute of every species of merit. . . . The paper which stands first is another lecture, containing more fancies, more blunders, more unfounded hypotheses, more gratuitous fictions, . . . and all from the fertile yet fruitless brain of the eternal Dr. Young. In our second number we exposed the absurdity of this writer's "law of interference," as it pleases him to call one of the most incomprehensible suppositions that we remember to have met with in the history of human hypotheses."

There are whole pages of it, but this is enough; and I cite this passage among many such at command, not only as an example of the way the undulatory theory was treated at the beginning of this century in the first critical journal of Europe, but as another example of the general fact that the same thing may appear intrinsically absurd or intrinsically reasonable, according to the year of grace in which we hear of it. The great majority, even of students of science, must take their opinions ready made as to science in general; each knowing, so far as he can be said to know anything at first hand, only that little corner which research has made specially his own.

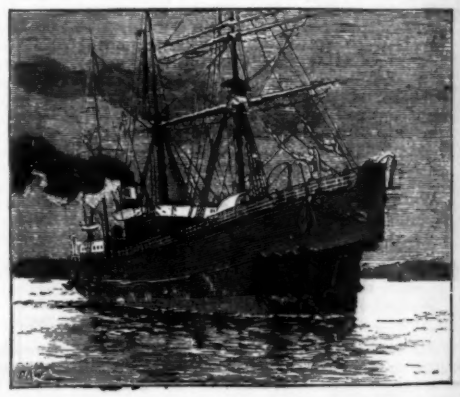
The moral we can all draw, *I* think, for ourselves.

In spite of such criticism as this, the undulatory hypothesis of light made rapid way, and carried with it, one would now say, the necessary inference that radiant heat was due to undulations also. This was, however, no legitimate inference to those to whom radiant heat was still a fluid; and yet, in spite of all, the modern doctrine now begins to make visible progress.

(To be continued.)

THE GEISER-THINGVALLA COLLISION.

ON Saturday, August 11, the steamship Geiser, of the Danish Thingvalla line, cleared from New York for Stettin with one hundred and forty-eight souls on board. A few days before, her sister ship of the same line, the Thingvalla, sailed from her home port, bound for America, with four hundred and fifty-five passengers. On August 16, the steamship Wieland, of the Hamburg-American line, brought into New York thirty-one of the Geiser's company—all that survived—and all the



passengers of the Thingvalla. Of the two Danish vessels which had come into collision off the Nova Scotian coast, the Geiser lay at the bottom of the ocean, and the Thingvalla with her bow literally broken off, and only her crew remaining on board, was slowly making her way toward Halifax, which port she ultimately reached. In the collision, the Thingvalla struck the Geiser almost at right angles, cutting her nearly in half, and sending her to the bottom in less than ten minutes, with one hundred and seventeen of her passengers and crew. Our illustration is from a photograph showing the Thingvalla as she appeared when entering the harbor of Halifax.—*The Graphic.*

ON TORTOISE (CHRYSEMYS PICTA) WITH TWO HEADS.

By E. H. BARBOUR.

THE following is a brief description of a young two-headed tortoise, *Chrysemys picta*, which is a remarkably interesting specimen from the very perfection of its imperfection. It was found in the marshes bordering West River, in New Haven, Conn., by Master Leighton Foster, the first part of June, and from appearances had been hatched but two or three days.

The single body, with the usual four legs and a tail, carries two equal and in every respect normal and well-developed heads and necks, which are throughout entirely free and distinct. The two heads see, hear, eat, drink, sleep, breathe, and move independently. See Figs. 3 and 4, and compare with Figs. 1 and 2, which represent a normal tortoise of the same age—about two weeks. The carapace, to external appearance, is well formed and natural, save that it is broader than long by nearly one-fourth, and its vertebral line is a little curved to the right, with the back slightly humped. But none of these are noticeable deformities, had it a single head. Its appendages are perfectly natural so far as their form is concerned.

This little monster still lives (Sept. 4), and has increased in size at least one-third during the past fourteen weeks. It is active and apparently healthy, and bids fair to survive this season at least. It eats voraciously when fed by familiar hands; but a fly or cricket is often a bone of bitter contention. The first to seize its food becomes at once involved in a stubborn tug of war with its other self, which ends only when the morsel separates. The two heads eat with equal readiness, yet often the appetite of one is greater than

that of the other. Sometimes one head turns slowly around and snaps at the yellow eye of the other, obviously mistaking it for something to eat. Of course the head and neck of the one assailed is straightway withdrawn into the common protecting shell, where they can find shelter, one at a time or together; but in the latter case it is plainly crowded, and the encroachment on tortoise prerogatives means a renewal of hostility, and a beating of their heads together till a compromise is effected. These little misunderstandings are always settled in a spirited way, and are exceedingly ludicrous.

Although rather frequent, they never arise save when the two heads are disposed to withdraw to their shell simultaneously; at other times each in his turn enjoys the privilege. One often withdraws and sleeps while the other is perfectly wide awake. And then the one awake, looking about to the right and left, sometimes starts off vigorously, but only to find itself describing a circle, round and round as if on a pivot, for such, in fact, the sleeping side actually becomes. And generally it continues to use its two feet as best it can, scurrying around in an endless circle, until the sleeper, aroused by the commotion, puts out its head, looks about, and then shuffles off with its companion. There is no concerted action whatever in the use of the feet, as in the normal tortoise, which first puts one fore-foot forward and follows this with the diagonally opposite hind-foot, and so on. But, as should be expected, the two-headed tortoise, with its two ambulatory systems, puts out both fore-feet at once, leaving its fore-parts without support, so that they drop and rest on the plastron; then the hind-feet advance, and the hinder extremities, left in their turn without support, drop, and thus it advances by an awkward rocking gait.

But these twin heads have finally learned to adapt themselves in various ways to their circumstances. This is especially striking in the matter of walking. By repeated failures, each had discovered that when its companion sleeps, or is not disposed to move, any activity on the part of either is circumscribed by the narrow circle whose radius is the breadth of their common body. Accordingly, now, when either has made a few bootless revolutions, it stops short and, extending its two feet laterally, seizes with its claws anything offering resistance, and so slowly and laboriously drags itself sideways, crab-like. By this device, to which they resort so repeatedly that it cannot be counted mere chance, they can travel on indefinitely. When placed on smooth ground, free from grass and obstructions, their first maneuver, after turning the right head to the right, the left to the left, is to start off resolutely in these two directions at once, the rather remarkable resultant of the two forces in opposite directions being a straight line directly backward: they make progress for a foot or two in the wrong direction, then agree on a course and start off together. In the grass or weeds they are quite helpless, because when a stalk of grass is encountered, one head chooses the right course, the other the left, that is, they straddle it, and being equals in strength, neither side succumbs, so they stand there tugging away until tired out.

It is interesting, though not surprising, that two heads so nearly one should have different temperaments. The right head, on most occasions, is the more timid and irascible, retracting or dodging at a passing fly or the approach of a strange animal. The left head, on the other hand, seems bold and energetic. It is difficult to conceive of any two individuals growing up under surrounding circumstances more completely identical than these. Yet, like the South Carolina negresses, called the two-headed nightingale, the difference in their dispositions furnishes new evidence that, though the origin and environments be precisely the same, the results are not necessarily so.

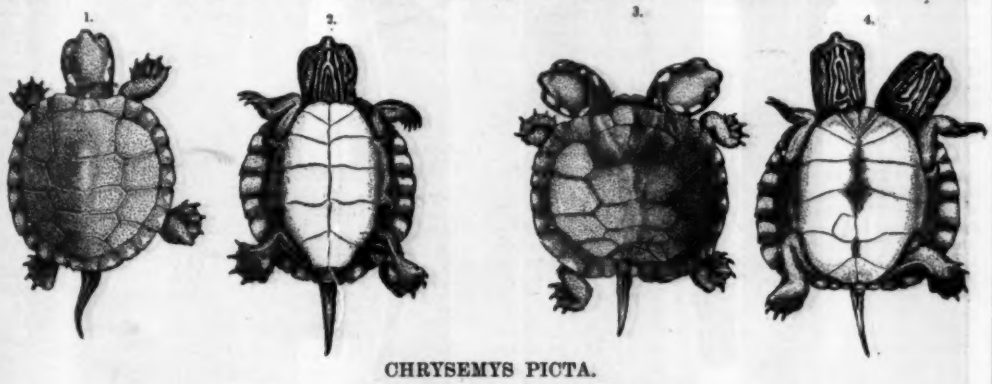
To what extent the digestion, respiration, circulation, and nervous systems are united or separated is at present only open to conjecture. The alimentary canals, in all probability, become united in one stomach after leaving the two necks, and remain so to the anus, which is single. I have noticed that while the two heads eat equal amounts, yet at one feeding the left head, perhaps, shows the greater appetite, at another time the other. But no risks can be taken by experimenting with such a pet, or we could feed one at the expense of the other for a short time. By watching the expansions and contractions of the throat, where the hyoid plays so important a part in chelonian respiration, we find that each of the heads breathes regularly, but independently, as much so as if they belonged to disconnected individuals. At regular intervals they can be seen opening their mouths and gaping, as if the supply of oxygen was insufficient, as it doubtless is. This is their only visible sign of weakness.

As already stated respecting the nervous system, it is perfectly patent that the appendages of the right and left sides belong to, and respond only to, their respective heads. So noticeable is this independence in the action of the four feet, which otherwise seem to belong naturally enough to the one carapace, that many who see its attempts at walking are led to "wonder if the other legs of the double shell are not growing inside."

There seems to be absolutely no co-operation between the left side and the right, and yet they repeatedly start at the same time to do precisely the same thing, eat, swim, or walk. When by any mischance it falls over on its back, the two heads work in opposite directions to right the shell, and so without help it could never, like an ordinary tortoise, extricate itself from the predicament.

The length and flexibility of each neck shows it has the full number of cervical vertebræ, confluent at the first thoracic vertebra. In other respects its skeleton is quite normal. The dermal plates show a few unimportant variations. There is the usual nuchal plate in front on the middle line, and two pygal plates behind. Between these two points on either side are twelve marginal plates; the usual though not invariable number being eleven. On the right side an extra scute is wedged in among the costal plates, making five on that side to four on the other. The five vertebral or neural plates preserve their relative positions, but have somewhat distorted forms. The first of these is divided by a suture through its length. The fifth is composed of four small, irregular plates, and presents a fissure where it did not unite on the middle line. The plates of the plastron show but one irregularity, a doubling of the gular plate. A suture is seen in the right femoral plate; and the right infra-marginal plates are united, the left separate; but these are not peculiarities however.

When the time arrives for the promised dissection of this curious little monstrosity, I shall hope to find anatomical peculiarities of some consequence.—*Amer. Jour. of Science.*



CHRYSEMYS PICTA.

Figs. 1, 2, normal form; 3, 4, form with two heads.

SOME EXPERIENCES WITH BIRDS.

MY first experience with caged birds was with canaries. I had several of these which I kept in gilt cages, hung up in the window. The cages were provided with seed, water, and cuttle fish, but nothing else. On the bottom of the cages, instead of sand, was a clean piece of paper cut round to fit, and changed each day. The birds were afraid of me—I never talked to them—and it was a trying task in the morning when I attended to them; especially trying was it for me to put my hand in at the door in order to remove the bath tub, for it always frightened the poor things almost to death, and I was often tempted to leave the door open and set them free, not knowing, in my ignorance, that they would be quite incapable of finding a living out of doors, after being reared in cages. To me a bird was a bird, *i. e.*, a creature with wings, and I thought he ought therefore to fly at his pleasure whithersoever he would. In time my birds died one after the other; having no sand, one of them got crippled feet, another, from hanging in a draught, took a cold from which he never recovered; still others died from one accident or another, and these experiences discouraged me altogether in trying to make pets of birds. They seemed to me unhappy in their prisons, and I could not, long enough, forget the injustice of holding so free a creature in confinement, to be able to enjoy either their pretty ways or delightful songs; and I felt too much compassion for them and had too much respect for their wild nature to make any efforts at taming them or in teaching them not to fear me, believing that every effort of mine in that direction would be simply a species of torture to the poor little animals themselves. Thus I was deprived, for many years, of a pleasure which I now consider not only lawful, but beneficial to a great degree; for the study of and companionship with the least of God's creatures is ennobling to man, and birds are by no means the least; on the contrary, I consider them the most beautiful and charming of all the animal creation.

The first person who introduced me into the real bird world and drew my sympathies into close connection with the songsters of the air was my husband. He procured a starling, and, by my advice, clipped his wings. I say by my advice, for since he insisted that the bird would become so accustomed to us that he would, in time, eat out of our hands, I was in great haste to bring about this happy consummation, and to experience the joy of having a bird which could understand the kindness of my feelings toward it and which would not be afraid of me. So we clipped poor starling's wings and gave him a perch in the corner of the kitchen, where his food and water and a box of sand were placed. But he had a fancy for wandering in the bedroom and parlor, both of which were too nicely furnished to admit of this familiarity on his part; then he was fond of jumping on the stove, which resulted in his feet getting badly burned one day. So that we finally removed him to a small room of his own and shut him in. My husband, who is an enthusiast in whatever he undertakes, was not satisfied with procuring me a starling. He purchased two or three canaries, a mulet, several small African birds, and a paroquet, and I began the study of birds in real earnest (I was going to say with a vengeance), for it was considerable of a task to attend to them all in the morning, especially as we were constantly changing them about from cage to cage in order to experiment with them. My sympathies were often outraged in this—what I then deemed—cruel exercise, and many an indignant expression escaped my lips when I saw the little creatures disturbed and frightened day after day, all for our pleasure. But they soon became so accustomed to it, that they would scarcely be out of the hand (which held them tenderly and without hurting them in the least) before they would break out into loud and joyful song. It took me a long while to learn how to catch and hold a bird without awkwardness and danger, for the least pressure is injurious to these tender little creatures; yet I learned it in time. Our birds all became very tame and very fond of us in the course of a month, and Mr. Starling was permitted to make a visit once a day to the rooms where the smaller birds were kept, and they soon came to know him and not to be afraid when he walked over the tops of their cages; but generally, after visiting them each in turn and pecking at their food through the wires, he would mount upon the mantels and begin to admire himself in the glass, when, of course, he had to be removed again to his lonely room. When we had owned him for about a month, and had given up all hope of his whistling for us, my husband called to me one day to step softly into the hall. I did so, and what was my surprise to see the bird (the door of his room being slightly ajar) with his neck stretched at full length uttering the most comical medley of strange sounds I had ever heard! I was charmed, and ventured further out into the hall where the bird could see me, for I wished to try whether or no my presence would disturb him. I thought of course it would, and expected to see him sneak away into his box, where he was wont to retire whenever any one approached; but my expectations were deceived. When he saw me, the vain creature, evidently flattered by the interest which I took in him, stretched his neck still further and continued his weird performance with renewed zeal; first giving a long, loud whistle in an extremely high key, then a rattling noise came from his throat, like the sound of water when the faucet was turned on, and this he kept up for a full minute, then gave one or two sweet notes, followed by a series of sharp little barks, perfectly resembling those of a small dog that was frequently heard from the rooms adjoining ours.

These accomplishments must have required considerable practice, and the bird must have been exercising secretly for some time in order to give us a surprise; for we had never heard so much as a note uttered by him before; and he had constantly played the part of a bashful and sulky bird, always hiding from us and running away on every occasion as if he disliked our company; while from that moment he was as amiable and friendly as any bird could be. It was a long time, however, before he learned to take worms from our hands, and, in fact, he never did it willingly. My husband would press one finger on his toes, which he disliked so much that he would jump upon the finger (being cornered and not daring or rather knowing it was useless to try to get away), and when perched upon the finger he would then—also, because he believed he would not get it at all—consent to eat a meal worm.

He proved to be a very hearty bird, for after this we were obliged to keep him in a very warm room for some time and then suddenly change him to a cold one in midwinter, which I feared would kill him, but from which he seemed to suffer no inconvenience whatever. This cold room was composed of glass, being meant for a conservatory for plants. Toward spring we put a great number of plants in it and also several birds; allowing them to go in and out of their cages among the pots as much as they pleased. The starling preferred his cage mostly, as it protected him from the annoyance of having his toes pressed, the perch being too high to render that pastime possible, and he only came down to take a look at the cats now and then, several of which used to climb upon the window sill and, with swollen tails and wild eyes, glare in upon the birds. The small birds always flew up and hid on the ledges of the woodwork at the top of the conservatory when the cats made their visit, but the starling was sure to come down and walk leisurely about, as if most unconcerned, and as if taking special delight in teasing the enemy, knowing full well that the glass was a perfect protection. There were folding glass doors opening from the conservatory into the back parlor, and other folding doors from this room into the front parlor, and from the latter room I was able to see, without disturbing, all the movements of the birds.

In the month of May, Mr. Starling walked out of the conservatory window, which, by some accident, was left open, and though he wandered for some time about the neighboring gardens, I was unable to coax him back into the cage. This distressed me exceedingly, as I feared he would not be able to fly, for his moulting time had not yet come round, and his wing feathers had had no chance of growing out since we had clipped them. But after an hour or so, he mounted to the highest branch of a tree that stood in the garden, where he whistled a succession of sweet notes which he had lately learned of the robin, and I then had the pleasure of seeing him fly off to a great distance, quite as easily as if he had had his wing feathers renewed; and as sorry as I felt to lose him, I could not but rejoice for the poor bird's sake that he was again enjoying the bliss of freedom. My husband, of course, was as sorry to lose him as I had been, especially as he had afforded us much amusement latterly by his antics in imitating the paroquet. His admiration for this haughty bird seemed to be unbounded, and he would follow him about, perching beside him on the cage, imitating his postures to perfection, and drawing in his feathers and dodging out of the way if the parrot, by accident, turned his head toward him. No doubt, he would, in time, have learned his noisy language, which would have been anything but agreeable for us. As for the paroquet, we never became attached to him, because he persisted in his disagreeable ways to the last, and refused to make friends with us or to take the least notice of any of the smaller birds. He would march about the rooms, ignoring everything and everybody, throw out all his food and snap at us if we came near to him. I once snatched him from the fire, and no doubt saved him from a horrible death. He showed his gratitude by refraining from biting my finger, when I placed it in his mouth, but he still continued to snap and pretend he would bite, on every occasion. A little robin was the favorite of all our birds, and as he had been for nine whole months confined in a very small cage before my husband, seeing him, took pity on him, and brought him home, where he immediately set him free, the little creature seemed never to be grateful enough, but to consider it his duty to show his gratitude whenever he could. He

also undertook to dictate to us and to take upon himself the responsibility of caring for the other birds, especially those that were kept like the tiny Africans in cages. If he thought we bothered them too much, he would begin to scold, but if they were attended to, in his opinion, correctly, he would perch upon their cage and sing. He slept upon a little branch of holly which was fastened at the head of our bed. He would wait patiently in some other place until our usual bed time, when, if we made no move to retire, he would begin vehemently to scold, always curtseying at each tschip! tschip! by bending his head low and lifting his tail high in the air. He always came down to eat at meal time, was very fond of nipping bits of cheese from the plate, and when he had no appetite he would pretend to eat, fixing his eyes upon one of us to see whether we were pleased. If I held a meal worm between my fingers, he would come from the farthest corner of the room and light upon my arm and take it from my hand. Sometimes when my husband was reading, he would perch upon the book or newspaper. Especially was he pleased when my husband came home with parcels in his hand. The robin would come down from his high seat and light upon each parcel or walk around them on the table, singing to each separately.

Finally we parted with all of our birds except the robin, as we had to move and found it a great trouble to take them with us. The robin was delighted, and sang much louder and came to us much oftener than before, for he was very jealous of the other birds, especially of a mulet and padda which we had added to our store. About this time, too, he fell in love with his own image in the glass, and would throw back his head and sing the most wildly enchanting songs to it. He came to understand everything we said. If, for instance, I waved my hand toward the back parlor and said, "Go in, Bobiny," he would immediately obey. This dear little bird died while moulting. I found him one day lying at the bottom of the aviary. Probably he had a fit. We were never as much attached to any other bird as we were to him, for his ways were exceedingly winning and sweet. Yet with all this he once acted very selfishly. Some one had taken two poor baby robins from the nest and afterward, not knowing what to do for them, had brought them to us. We put them on the floor, and they immediately began following Bobiny about the room, screeching with wide-open bills and their wings quivering like a leaf fluttered by the breeze. We placed some meal worms within reach, thinking our bird would feed the young ones with them, but he ate the worms up, regardless of all protests from the orphans, whose screams and flutterings became every moment wilder. Presently he selected a dead worm from among the live ones in the jar, and took it to one of the infant birds, which, of course, was unable to eat it, and having apparently satisfied his conscience, flew up out of their reach and refused to have anything more to do with them. They were both dead the next morning, to our sorrow.

We had two robins after we lost this one, one of which we kept in the aviary, while one flew about the room. They used to amuse themselves by fighting through a hole in the bottom of the cage, just large enough to admit of their heads reaching each other. The linnet has ever been a favorite of mine since I first heard his song, which is more cheerful than that of the robin and has more variety; but the linnet is shy and does not become easily attached to human beings; nevertheless, we have had two linnets which became very much attached to us. One of them died from the effects of a broken leg, and one we were obliged to leave with friends on our departure from England. The latter we had for three years in a very large cage, destined to hold several birds, and of course we made a great pet of him. He used to answer me when I spoke to him and we held many conversations together during the long, tedious hours when my husband was away on business. At last we got him a companion; this was a little goldfinch, of a very lively and sweet disposition, and they lived quite happily together. But if I tried to talk with the new bird, the linnet would look at me with the most unmistakable expression of jealousy and wounded pride, and in a tone which was decidedly reproachful, begin to complain and to chase Finchy about the cage. This linnet was terribly afraid of cats, and although he was a magnificent singer, he nearly lost his voice, on account of the frights he had at several different times from the cats, that abound in every house and street of London.

The chaffinch seems to be an amiable, lively bird, but we had ours too short a time to study his character to any extent. However, we discovered that he was almost as jealous of other birds as the robin, though not half so sensitive, singing at the top of his voice all day long, while the robin, on the contrary, would frequently pout for hours when something happened to displease him. Among the tiny African birds which we kept, there was one which sang a very pretty song, starting at a high note and running down into a very low key. He was not larger than a walnut, but very straight and proud. We liked these tiny birds mostly for the endearing way they had of all snugging up close together on the perch whenever they were not eating or bathing, and they were beautiful on account of the richness of their plumage, which, in some, was of a shaded orange, in others of a scarlet dotted with white and black.

The chief things necessary to success in keeping tame birds is to keep their cages clean, with plenty of sand on the bottom and a good supply of bath tubs. The sand is necessary to preserve their feet from scales, and the bath for their general vigor and health. Then every bird has a preference for certain kinds of food, which should be given him in moderate quantities and kept in a dry and clean place. Some birds, like the goldfinch, need entertainment, and it has given me many a pleasant hour to watch this beautiful little bird pulling to pieces certain flowers, such as the sweet pea or the corn flower, in order to get at the honey concealed in their folds.

The goldfinch is very handy with his feet, and uses them both for climbing about like a monkey, and also for holding anything, like a bit of watercress or piece of chickweed, while he is eating it. His song is low and sweet, not brilliant. One other bird, the Java sparrow or "padda," we liked very much. His song is peculiar; but we found him an interesting bird on account of his intelligence and affectionate disposition. We would hear his weird song at the first break of day, and long before that of the other birds, who mostly preferred eating their breakfast before giving thanks. The padda, however, was always ready to sing, and his earthen-like notes, although they sounded strangely among the melodious songs of the robin, the mulet, the canary, and the linnet, were never unpleasing to the ear.

The canary is the least interesting to me of all caged birds, for he seems not to sing from the happiness of his heart, but rather from a desire to display his talents, while he is too selfish or too spoiled to form very strong attachments. We had one green canary, however, which had been caught in a wild state, and we found him very different from those which had been reared in cages. We cured him of crippled feet by keeping him for an hour every morning in a traveling cage standing in lukewarm water. His song improved wonderfully after his feet were well, and he became greatly attached to us. He was very fond of being talked to, and would not permit his mate (a yellow female) to respond, but quickly shut her up if she made any effort, by pecking her on the head. This female canary he liked exceedingly, and would cry and show great distress if she left his side, yet he wished her to keep in her place, and gave her plainly to understand that it was not becoming for her to reply when she was not spoken to.

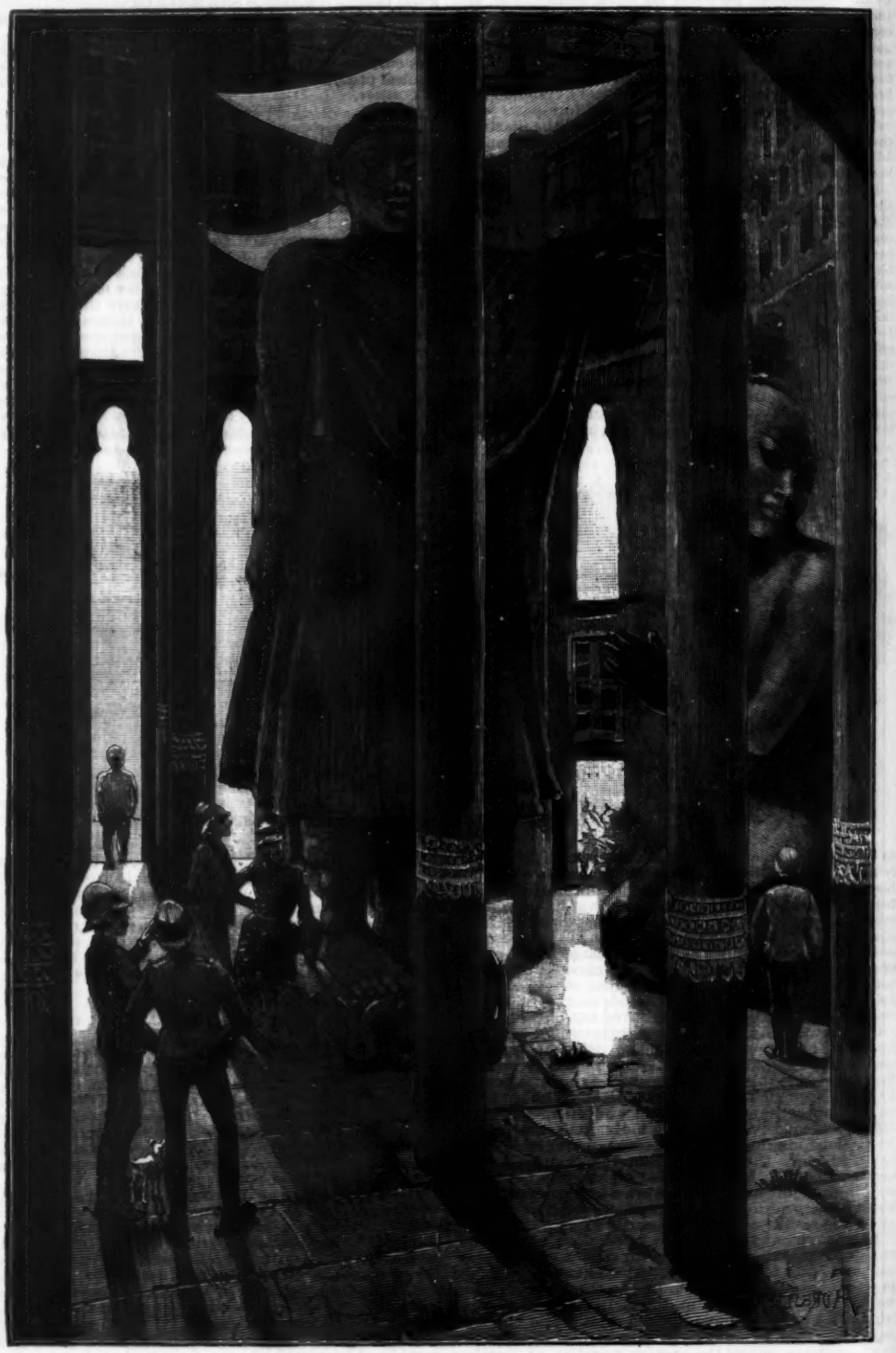
We have kept both the thrush and the lark in cages, and found them very charming birds. The thrush was taken from the nest, and proved to be of an exceedingly sentimental disposition, singing unconsciously when all was confusion around him, and never seeming to be aware of his confinement. He once or twice escaped from the aviary, which was not a very large one, and went tumbling about the room in the most awkward manner, not knowing what use to make of his freedom. It was very painful for us to part from this bird; but ill health necessitated our giving up our rooms in London and going to the seaside; and we found it impossible to take any birds with us there.

The skylark, of which there are hundreds kept in cages in London, and may be heard singing all winter, as you walk through certain streets, we kept but a short time imprisoned, for it seems exceedingly cruel to keep this bird, who so loves to soar heavenward and seems to mount upon the very wings of his song, in confinement at all; and in Germany, where birds are most loved and studied, it is almost unheard of. However, we found to our surprise that the lark soon became tame, and seemed contented and happy. We enlarged his cage so that he could walk about, bathe in the sand and rest on a bit of turf provided for that purpose. He sang for us in the course of a few days, but being one day frightened by the cat, he stopped singing, and we then concluded that we would be happier if we were to let him go; so we set open the door of his cage and he mounted high in the air and was soon out of sight.

This is a brief report of my experience with European birds. With American birds I have as yet had no experience, but hope to become acquainted with many of them before we return to the old country. DE C.

STATUE OF BUDDHA.

THE hill that rises behind the city of Mandalay is



COLOSSAL FIGURE OF BUDDHA ON MANDALAY HILL.

covered with sacred groves, monasteries, pagodas, and temples of Buddha, occupied by at least ten thousand servants of religion, most of them living in convents, but not without plenty of wives and children. Some of the temples are very large and of stately architecture, constructed either of solid masonry or of teak timber, elaborately carved and gilded. The one called "The Incomparable," which is unfinished, standing on the summit of a series of square terraces, is entered by gilt doors, and contains two lofty halls, having pillars all round coated with gold leaf, which glitter in the light of many chandeliers of stained glass, apparently of European manufacture. The seated image of Gautama Buddha in this temple is 30 ft. high and is gilt all over; a large diamond was formerly stuck in the forehead between the eyebrows, but this gem has been stolen. There are many other images of the same divine personage at Mandalay, and hundreds of shrines

contain sacred figures, or tablets inscribed with the records and precepts of the Buddhist faith. The phoongyes, who shave their heads and wear yellow robes, possess great collective wealth in the landed estates belonging to their various corporations.—*Illustrated London News.*

HOW PLANTS ARE FERTILIZED.

PROBABLY most of us are familiar with the pollen of plants, however little we may know of its nature and use. Even the most unobserving of people can scarcely fail to notice the showers of yellow and reddish pollen shed by the lilies, and many a one has had a very close acquaintance with it, when, in his eagerness to catch the odor, he has put his face too close to the flower. In fact, so abundant is the pollen, and so easily shed, that florists clip off the stamens before placing the lilies in the market. To the naked eye the pollen appears like very fine dust, usually yellow or brown, but occasionally whitish or even pink. It seems to be the same in size and form in every flower, so that one is almost entirely unprepared for the wonders revealed by the microscope. Under a powerful lens the yellow dust is seen to be made up of myriads of tiny globes, some nearly transparent, others filled with a granular substance. They are of a great variety of shapes, hardly any two being exactly alike. Some are round, others oval; some have their outer coat smooth, while we find many covered all over with prickles, as in the asters, for example. Others show belts of membrane covered with sharp points. The most common forms show three of these belts. In the accompanying drawing, Fig. 1, this may be seen in the spring beauty, *a*, oxalis, *c*, and willow, *h*. The common succory (*Cichorium intybus*), a familiar blue flower in many fields, and a nuisance to boot, has globular grains, each with a network of ridges around it, each ridge rising into jagged teeth on the outer edge.

Another curious form is that seen in the bear berry, *d*, and, it is said, in all other members of the heath family. Here we have four smooth grains united into one. Again, some grains show a central body with several small projections from the angles. Such grains may be seen in the evening primrose family and in the Shepherdii, *f*, among other plants. In some the projections may be larger than the central mass. Other common forms, seen in the accompanying illustration, are the trillium, *g*, hyacinth, *e*, and poplar, *k*. There seems to be no end to the variety of forms of the grains, though in many cases the species of the same genus seem to be nearly alike. Still this may have but little significance, for some plants of the most diverse families have pollen grains very similar. Just why these particular forms have been developed is a puzzle.

The roughness of the outer coat of the grain was mentioned above. That brings us to a more minute study of the structure of the grain. We find it to consist of a sac surrounded by two coats, the outer of which is always nearly or quite perforated in one or more points. The interior is filled with more or less granular and dissolved food matters, such as starch, oil, sugar, and dextrin. A nucleus can generally be found in the grains, and in some two.

As to size, they vary greatly, though the grains of the same species are usually pretty uniform. The largest yet examined by the writer were those of a "snake cactus," which were about 1-265 of an inch in diameter. The smallest were those of the common hazel nut, which average only 1-1050 of an inch.

A very interesting fact is that some plants have two or three forms of flowers, differing principally in the length of the style and of the stamens, and that pollen from the two forms differs in size and perhaps slightly in form. More wonderful still is the fact that the pollen of the short-styled form has very little if any effect on the flowers of the same sort, while it readily affects the stigmas of the long-styled form.

We know that the work of the pollen is to fertilize the undeveloped seeds or ovules in the pistil, but how many can tell how it is done? About the time of flowering there appears in the ovule a little sac, lined with delicate membrane, and containing a tiny globule of gelatinous matter—the germ of the future plant. Now this germ has, in itself, no power of growth. It needs to be fertilized by the pollen or it never develops.

FIG. 1.—COMMON FORMS OF POLLEN. (Largely Magnified.)

But the question at once comes up, how does the pollen get at the ovule? It can be seen sticking to the stigma, but that is often a long way from the ovules, and raised high on a slender style. Since it cannot get down to the ovule, it does the next best thing. From its inner coat grows out a tube, long or short, as the case may be, and through this flow part of the contents of the cell. This tube, as it lengthens, pushes its way down through the substance of the style till it comes to the ovule, and at length reaches the sac of which we have spoken. Here its contents become united in some way with those of the germ cell, which now begins to grow. The first step in this growth is a division of the single cell into two. These new cells keep on forming in the same way till we soon have the perfect form of the plant in miniature, still in the ovule, which has now become a seed, capable of producing the new plant as soon as it is placed under proper conditions.

In some plants the pollen tube must gain a great length in order to reach the germ cell. One of the best examples is the "Indian corn." In this, as every one knows, the tassel on top is a cluster of many small flowers, bearing only stamens, while the cob with its grains represents a spike of ripened fruit formed from flowers bearing only pistils. The silk is the style, and in order to reach the grains, the pollen must fall on it and must send its tube down through the whole length. It is evident that the grains at the base of the cob have much longer styles than those at the tip, but, being on the outside of the tuft of silk, they stand a better chance of catching pollen grains. The grains at the tip are

FIG. 2.—POLLEN GRAINS OF WILLOW, SHOWING TUBES.

usually less well developed than the others, partly, perhaps, from their unfavorable position in the cluster of silk and partly because they mature later than the others. Notwithstanding these facts about corn, we can readily see how a hard rain storm, when the corn is just in tassel, may ruin the crop. The rain washes the pollen from the styles, and by the time more reaches them they have passed their receptive stage. The germ cell, then, never develops, and the ovules never become seeds.

A still more wonderful plant, at least as to the growth of the pollen tubes, is the night-blooming cereus. Those who have seen this know that down its pure white bell reaches a snowy white style five or six inches long, perhaps more. Of course the pollen has to send its tubes the whole length of that. Now the wonderful thing is that, inasmuch as the flowers last only a single night, the pollen tubes must grow to the ovule in the course of a few hours at most, since, as a rule, flowers do not wither till their work is done.

Any one who has the use of a compound microscope can easily watch the growth of pollen tubes for himself. There is no difficulty at all in making them grow. All that is necessary is to shake the pollen from the flowers into a solution of cane sugar, and set it in a moderately warm place. Then examine it carefully at frequent intervals with the microscope, and you can trace the different stages. Or you can sow a new lot each day and get all the stages before you at once. The writer has had greater success with native plants than with cultivated ones. A plant whose pollen shows very vigorous growth is the willow. Some grains sowed in the evening were by morning literally matted together with long tubes, so that it was almost impossible to draw them. Fig. 2 shows some of the stages in the growth of willow pollen, quite highly magnified.

Another point of very great interest, but one which we can touch only briefly here, is the manner in which the pollen is spread. In many plants it is carried by the wind, especially in trees and shrubs which bear thin flowers, high in the air. These we find, as a general rule, have small and inconspicuous blossoms. In most plants, however, the carrying of pollen is performed by insects, and to that end flowers have their bright colors, while there is no end to the wonderful contrivances for letting in the right insects and keeping out all others.—*L. N. J., in Country Gentleman.*

A BOTANICAL ATROCITY.

By MILLER CHRISTY.

AMONG all the many ingenious contrivances for effecting her various ends which Nature makes use of, none are of a more fascinating interest to the student than those by means of which the flowers of plants are fertilized and their seeds distributed over the ground to spots where they may hope to obtain a foothold—or, to speak more correctly, a roothold.

We are all of us accustomed to speak glibly of the "struggle for existence," and we are apt to think that nowhere throughout the whole wide range of nature is this struggle more severe than in the case of our own exalted species, in these days of excessive "competition" and "over-population." But a little reflection may well lead us to doubt whether this is really so, and even to perceive that the struggle is much keener with many other species than with our own. How many, for instance, of the nine millions of ova found in the roe of a single codfish ever produce a mature fish? How many, again, of the tiny wriggling tadpoles that blacken the margins of our country ponds in spring ever attain maturity as full-grown frogs? And what proportion of the seeds produced by any one plant or tree ever finds itself so fortunate as to be able to take root and grow, unsmothered by the surrounding vegetation? Probably, in all these cases, not the one hundredth part survives to reach maturity. Were it otherwise, indeed, the ocean would quickly become packed solid with codfish, the land overrun with frogs more numerous than at the time of the second Egyptian plague, and the surface of the soil entirely concealed by a dense mass of impenetrable and tangled vegetation. An "infant mortality" so prodigious, in our own species, would certainly appal even the soulless official mind of a registrar-general.

If, then, the struggle for existence is so severe in nature, it can hardly be wondered at that many exceedingly ingenious contrivances have been, in the course of time, devised by plants in order to insure the wide dispersal of their seeds, so that each may enjoy as good a chance as possible of alighting upon some spot where there is sufficient room for it to germinate into a healthy plant. It may be truly said that, as a general rule, with all living things the chief aim and object of their lives is the perpetuation of their species by means of offspring. With plants this is well shown by the extreme ingenuity of many of the contrivances for insuring proper fertilization, and the consequent production of good seed. And it may, I think, be said with equal truth that second only in importance to this great function, the production of seeds, comes the proper dispersal of those seeds, to which end many almost equally ingenious devices have been adopted. Very many seeds are furnished with minute hooks which fasten themselves on to the wool of passing animals, and thus accomplish the great end in view; others are peculiarly adapted for dispersion by means either of wind or water; others are scattered broadcast after passing through the intestines of birds; while many plants have actually developed means for throwing their seeds to a greater or less distance. There are even some seeds which, in thus effecting their own ends, do so at the expense of living animals; but no known seeds, probably, employ means more atrociously and barbarously cruel than those employed by the seed of a species of grass known as *Stipa spartea*, which is excessively common over a large extent of the North American prairies. Briefly described, this villainous instrument is nothing more or less than an automatic vegetable corkscrew, with an exceedingly sharp point, and capable, by its own action, of boring itself into the bodies of living sheep and other animals, and of finally killing them by so doing.

The plant producing this "instrument of torture" has a very wide range of distribution in America. It inhabits the drier portions of the plains and prairies of the West, almost, if not quite, from the Mississippi River on the east to the Rocky Mountains on the west, and from Texas and Mexico on the south to the lonely Peace and Saskatchewan Rivers on the north. The great and only redeeming feature in its character is that, for purposes of pasturage, its leaves are more succulent and valuable than those of any other of the prairie grasses, and that, although too short and tussocky to be made into hay, they formerly provided the buffaloes with their chief winter forage, and now perform the same office for the tame cattle on many a Western ranch. On this account it has received, among other popular names, that of "buffalo grass"—indeed, its range in the west of America is pretty nearly coextensive with the ancient range of the buffalo.

During autumn, winter, and spring, the buffalo grass is in all respects a most moral and respectable plant; but early in the summer, it begins to plan evil ways. First of all it throws up three or four tall flower stalks, each about a couple of feet in height, and bearing six or seven of the horrible instruments of torture already mentioned. These ripen about the middle of July, and for the next two or three weeks make themselves troublesome, not only to the lower animals, but also to man, in districts where the plant grows abundantly. It now is fully launched upon its evil ways, and begins to deserve its other popular names, as "spear grass," "oat grass," "go-devil oat," "wild oat," "needle grass," etc. As one walks over the prairie at this time of the year, suddenly there comes a sharp prick, as of a pin, on the leg, just above the ankle. Stop one must, to investigate the cause, or the pricking will become more severe, and then the traveler finds that one of these seeds, and very often several, has bored its way clear through one's trousers and sock till it has reached the flesh. With man it is an easy matter to prevent the mischief going any further, but the case is very different with sheep, woolly-haired dogs, and other animals. Those cannot rid themselves of the "spears" the moment they begin to prick, and in time the consequences begin to be serious. There is no question that the seeds are able to penetrate the skin,

AWN OF STIPA SPARTEA (IN ITS DRY OR TWISTED STATE.)

and bury themselves deeply in the flesh of these animals. When this is done, death sometimes supervenes, though oftener as a result of the extensive ulceration thus set up, than as a result of the direct action of the seeds.

Next, a word as to the structure of these unpleasant natural tormentors. In general appearance a complete seed—or, to speak with greater botanical precision, a complete "fruit"—a good deal resembles a twisted piece of wire having several slight bends in it and being rather thicker in some places than in others. The seed proper is elongated, brown in color, somewhat less than an inch in length, and generally not very unlike a harmless oat in its appearance. At one end, however, it is provided with an excessively fine, sharp point, surrounded with many delicate barblike hairs, point-

ing backward; while at the other end it is produced into a stiff, slender awn, or shaft, rather less than two inches in length. This portion is usually quite straight, though always sharply twisted up upon its own axis, seven or eight revolutions being usually observable. At the top of the shaft there is a bend, usually somewhat larger than a right angle, which is termed the "lower knee," and in about half an inch another sharper bend, which has been called the "upper knee," after which the awn is still further produced into a stiff tapering bristle, somewhat less than three inches in length, entirely untwisted, and set with many exceedingly minute but sharp teeth, all pointing backward. The whole apparatus, for such it may be called, is thus about six or seven inches in length. A glance at the accompanying illustration, however, will give a clearer idea of the seed than the most elaborate description. The foregoing is a description of the instrument when in a perfectly dry state. On its becoming wet, the "hygrometric action" of the awn (as it is called) comes into play, and the appearance of the whole contrivance quickly becomes changed. The shaft then begins to untwist, the knees commence to straighten, and in a short time the whole awn becomes perfectly straight from end to end, so to remain until it is again dried, when the former twisted and bent condition is resumed. This curious power of untwisting and straightening itself when moistened, and of again twisting up when dried, seems to continue for an almost indefinite time—probably until the awn commences to undergo natural decay. It is a power which is also possessed, though in a less fully developed degree, by various other seeds. In most, if not all, of the members of the genus *Stipa*, in several other grasses, in some of the Erodiums, and even in the common wild oat, this curious hygrometric peculiarity of the seed is observable.

Let us examine how this power aids the seed in its nefarious designs. It is in this way. Suppose a ripe awn, in its dry or twisted state, to have become entangled in the wool of a sheep, or to have fallen among the grass. It there remains inactive, until moistened by a shower of rain or a heavy dew. Then, as we know, the shaft commences to untwist, rotating the point of the seed; for, as the bristle or arm of the awn forms at first a right angle with the shaft, it is certain to catch in the grass or wool, and thus prevent the head of the shaft from revolving as it untwists. But all this time, as we also know, the "knees" are straightening, raising the arm and causing it to press upward. In rising it will probably press against a blade of grass or catch in the wool, its many fine teeth affording it a more or less firm hold upon either. Under these circumstances, the continued efforts of the arm to rise will have the effect of pressing forward the revolving point of the seed, causing it to penetrate slowly and by degrees into the flesh of the animal, the ground, or any other sufficiently soft substance which may be opposed to it. Thus our automatic boring apparatus will be in full operation. Having penetrated to a slight depth, the barb-like hairs round the point prevent it being withdrawn when the awn retwists on again becoming dry; so there it remains, ready to penetrate further and further as often as succeeding drought and moisture cause the awn to twist and untwist again.

There is, as has been already said, no question that the seeds are in this way able to penetrate the skins of animals and actually enter the body. On the plains of the West its possession of this power is well known, and I have myself often seen and heard of instances of its effect. I have never, it is true, actually seen a sheep that had been thus killed, because sheep are not allowed to die of it, but are killed for the butcher. There is, however, not the least question that the seeds have the power of causing death, either directly or indirectly. A recent American writer, speaking of this subject, says that, after the seeds have penetrated the skin, "the awns break off, and the needles penetrate the vital part of the sheep, causing painful death. . . . The points, too, not only enter the body of the sheep in this way, but also stick in the nostrils, nose and lips, where, however, they do less harm than when eaten and swallowed into the stomach, in which event death must follow." I have been repeatedly assured when "out West," by butchers and others, that the "spears" are very commonly found beneath the skins of sheep after death—especially about the shoulders; indeed, I have myself seen them in this position, while I have been several times told by settlers of sheep they had been compelled to kill, "to save their lives," as Irishmen are supposed to say. I have, moreover, demonstrated that the seeds do possess this power by experiment in England. Some seeds, which I placed among the wool of two sheep, quickly penetrated to a depth of half an inch, after which I removed them, thinking that the animals were probably less interested than myself in the result of my experiment. Indeed, the keeping of sheep on the Western prairies would be rendered almost impossible, on account of the prevalence of this deadly grass, were it not for the fact that its evil effects can be avoided by a few simple precautions, such as a careful examination of the sheep during the time the seed is ripe, or "folding" them during the same period.

But sheep are not the only animals that suffer. Woolly-haired dogs are almost equally troubled, if proper care be not taken. I have heard of such dogs having, like the sheep, to be killed "to save their lives," when the whole of the breast and stomach has been found to be a mass of ulceration. I have also heard of a colly, which came so thoroughly to understand the nature of the "spears," that he would never enter the long grass while they were ripe; while one settler told me that he had found it necessary to shave his dog all over when the "spears" were ripe, to prevent their catching in his long matted hair.

Now, could any epithet be too strong to describe correctly a seed which conducts itself in such an atrocious manner? I think not. It must however be confessed that it is a little difficult to see exactly how the plant is benefited by its seeds being introduced into the bodies of animals. Their dispersal would, it is true, be usually assured, but the great object of that dispersal—namely, germination—would probably be deterred, unless it be that the seed has some strange power of retaining its vitality for a long period after its burial in the flesh of a sheep. It can hardly be supposed that the seeds are intended to grow there, or we should be presented with the strange spectacle of the sheep growing their favorite food on their own bodies! But this is too absurd. It may, perhaps, be that the simple and beautiful piece of mechanism possessed by the seed of *Stipa spartea* is simply to enable it to bury itself securely in the ground where it may take root—to sow itself, in fact; but if so, what shall be said of the plant which uses such a power for such murderous purposes as have been described! The whole matter is not yet quite cleared up.—*Knowledge*.

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